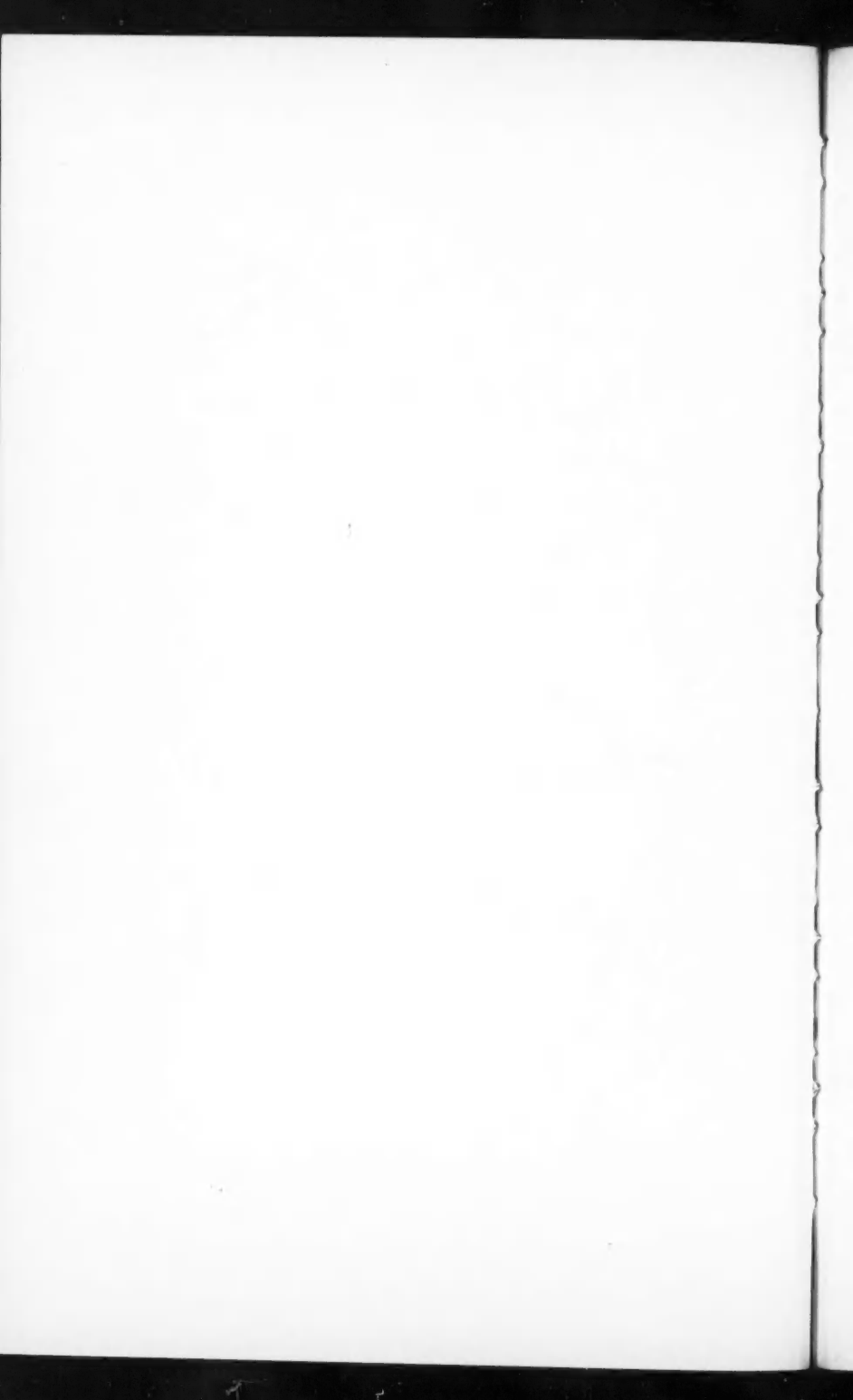


CANADIAN JOURNAL OF PSYCHOLOGY

VOL. 9, NO. 3, SEPT. 1955

PUBLISHED FOR THE
CANADIAN PSYCHOLOGICAL ASSOCIATION BY THE
UNIVERSITY OF TORONTO PRESS



CANADIAN JOURNAL OF PSYCHOLOGY

VOLUME 9, NO. 3

SEPTEMBER, 1955

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UNIVERSITY OF TORONTO PRESS

AUTHORIZED AS SECOND-CLASS MAIL, POST-OFFICE DEPARTMENT, OTTAWA

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THE CANADIAN JOURNAL OF PSYCHOLOGY is published quarterly in March, June, September, and December. *Annual subscription*, \$4.00; *single number*, \$1.00.

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3600 McTavish Street, Montreal 2, Que.

Contributions. Original manuscripts and correspondence on editorial matters should be sent to:

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100 St. George Street, Toronto 5, Ont.

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The Canadian Psychological Association also publishes *The Canadian Psychologist*, which is distributed to members only. *Editors:* JEAN GARNEAU and ERNEST POSER, Montreal.

THE CONTRIBUTION OF CLINICAL RESEARCH TO PERSONALITY THEORY*

NOEL MAILLOUX

Université de Montréal

THE INITIAL STRIVINGS of clinical psychology to build up a systematic theory of personality met with almost insuperable obstacles. The timid attempts at proposing new research hypotheses were frowned upon by those who were entrenched in the experimental laboratory. A rather queer situation emerged soon, where those who kept an uncompromising devotion for rats, mazes, and averages claimed full possession of the territory of scientific psychology; while those who felt a stronger interest in the study of human personality, individual and social behaviour, and human relations, found themselves relegated to a marginal field, considered as still half-clouded by magical thinking. The former, of course, were reluctant to see their well controlled laws and theories exposed to the puzzling evidence from everyday life. The latter hesitated to put aside the artificial, although useful, laboratory framework in order to develop the concepts and methods required for their own original approach, for fear of appearing less respectable scientists. Such inhibiting attitudes inevitably curtailed the enhancement of psychology as a unified and integrated knowledge of human behaviour.

To formulate hypotheses which would appear meaningful in his ordinary working conditions, the clinical psychologist had to proceed against the general trend of the time. No wonder, then, if he felt he wandered on insecure ground, being obliged to alter the dimensions of his problems until they could be camouflaged under the cloak of standard experimental procedures. Interesting conclusions were reached in this way, but the initial problems remained unsolved and frustration soon became inescapable. Evidently the clinical psychologist did not think of taking exception to the experimental method itself, which, as did every researcher, he regarded as an indispensable tool. With increasing awareness, he realized that the main source of his discontent resided in the basic postulates which guided its application. These, indeed, had come to be taken for granted and had, until recently, remained unchallenged, in spite of the fact that they were painfully restricting the areas of legitimate investigation. The unfortunate result of such apathy

*Presidential address, delivered at the Annual Meeting of the Canadian Psychological Association, Halifax, June 4, 1955.

can be seen in the all too prolonged conflict between philosophy and science. On both sides, the less sophisticated thinkers, forgetting that these postulates constituted nothing more than the temporary and still rudimentary rationale of certain laboratory techniques, got involved in unnecessary confusion. The philosopher looked down on the "simplistic" views of the scientist about the ultimate meaning of the universe or the nature of man, and remained suspicious of his narrowly materialistic bias. The scientist showed reluctance to give heed to the various aspects of reality which remained beyond his operational viewpoint and tended to regard any metaphysical speculation as mythical.

It is not my intention to undertake here a critical evaluation of the postulates which have been hampering for so long a more versatile and comprehensive use of the experimental method. This would lead to lengthy discussion, entirely out of place on this occasion. Rather, following G. W. Allport's (1) courageous and brilliant recent attempt to tackle at least part of this immense task, I wish to indicate the main points of divergence where clinical observation has forced the student of personality to part from the traditional approach. Following this, the most significant insights, arising from cumulative evidence, shall be proposed as landmarks of the new orientation which is increasingly prevalent in current research.

A first kind of limitation imposed drastic restrictions on the scope of the scientific method. It derived from the long accepted framework compelling the observer to look only in very specific directions. He indeed was expected to concentrate almost exclusively on three aspects of behaviour: its potential stimulus, its elemental components, and its genetic sequence. Thereby, he was to remain in full compliance with the basic presuppositions of contemporary empiricism, which attributes fundamental priorities to what is external and visible over what is not, to what is small and molecular over what is large and molar, and to what is early over what is late in development (1, pp. 7-12). As long as such an approach was used merely as a device for a more clear-cut analysis of factual data, regarded as readily accessible facets of a general and whole process requiring a much broader definition, no one questioned its fecundity in yielding useful and suggestive results. However, serious misgivings began to be aroused when it became biased by the contention of being a self-sufficient and totalitarian method, capable of supplying us with an exhaustive explanation of even highly complicated processes, such as those which take place in the psycho-therapeutic situation.

Certainly, the systematic exploration of variations affecting the determining, conditioning, or concomitant factors of observable reactions has

been a source of tremendous progress for the science of psychology. It has led to the designing of innumerable quantitative scales which now permit us to compare an individual with a whole population of similar background. Thus, from a utilitarian point of view one may rightly claim that a new realm has been conquered for applied psychology. Also, the precise determination of the elasticity of these variations and of the different degrees to which they happen to be correlated has not been without significant implications for general theory. However, one must admit that this leaves us still completely in the dark in so far as the specific causes of behaviour and the regulation of its manifestations are concerned.

Moreover, a too exclusive interest in the elemental aspects of psychological phenomena had undoubtedly nefarious consequences for the development of psychology. Any systematic study of the higher processes involved in human activity was promptly abandoned. The absence of general and comprehensive theory and of well articulated hypotheses which resulted from this serious neglect made impossible any further reinterpretation of painstakingly gathered factual observations. Soon we became confronted with an ever increasing aggregate of unrelated data, narrowly conceived interpretations and aggressively autonomous hypotheses, whose potential meaning remained desperately vague and elusive. In this state of momentary disorganization we lost sight of psychology as a unified science. Ceasing to focus our attention on its one core object, the understanding of human behaviour, whose various dimensions had to be explored systematically with the whole array of methods already at our disposal and the truly original ones which we should have invented, we missed the opportunity of building up a scientific synthesis of our own. As Salman (9, p. 20) pointedly remarks, each technique which proved to be useful for conducting a piece of investigation attempted to establish itself as a general theory, offering all the long-awaited explanations. To the eyes of a Freudian observer, psychology, as an "upstart" science, would undoubtedly appear as almost totally devoid of genuine "object love." And its fascination by methods and models currently in use in sciences which have definitely "come of age," such as chemistry, physics, biology, and mathematics, would suggest that it is still undergoing the immature stage of identification. This could have been a normal developmental phase of an integrated knowledge, but a too prolonged fixation seems to have confined us in an indefinite era of conflicting *psychologies*, which can hardly be exempt from any symptomatic tinge. Last year, Williams (11) drew our attention to these inevitable growth pains and boldly suggested a fresh approach. However, as we have been reminded by MacLeod's report (4,

p. 60), we have still to get busy dissipating the confusion generated around our subject-matter by a careless dispersion of our efforts and by what Erikson considers as a deplorable lack of identity.

Finally, a too shortsighted stress on the genetic approach has tended to obscure persistently the clear-cut distinction which should be maintained between growth and development. Being a natural and more or less rigid process which occupies only a limited period of our existence, growth can be regarded merely as a *conditio sine qua non* of our further development. The latter, on the contrary, depends on our individual initiative and may pursue its course unremittingly throughout life. Granted that one cannot emphasize too much the necessity of properly laying the cornerstone of this elaborate architectural construction, which will turn out to be a mature and well integrated personality, we must still avoid mistaking the fundamental role of the sustaining infrastructure for the fundamental generating process of the uprising superstructure. While the former is deeply implanted in human nature and may be expected to present a high degree of commonality, the second is bound to be mainly a product of the individual's spontaneous ingenuity and will likely display an unmistakable character of uniqueness. Again, we are justified in giving proper care to an indispensable precondition; but one would like to hope, at this juncture, that we will no longer let ourselves be absorbed in this preliminary task to the point of totally disregarding the essential dynamic principles of our individual development, whose implementation requires constant strivings toward higher achievements.

I would very much like to interrupt this rather depressing picture of the impediments arbitrarily imposed upon the broad and flexible use of the scientific method in our field. However, one cannot forget that, just as it was hampered in its move, psychology was crippled also in its contemplative activity to the point where it appeared almost blind to its specific and particularly relevant object—the human mind. Other postulates which were being advanced discouraged any investigation of the mind and even of consciousness as a datum, thus restricting progressively its whole sphere of interest to certain functions of the organism, if not to the ones of the nervous system. For years, a sort of regressive process has been going on, forcing psychology to withdraw from a vast territory unquestionably considered as its own from time immemorial.

To define his new approach without ambiguity, Wundt pretended to devote his efforts to "a psychology without a soul." Of course, this formula implied nothing more than a methodological option and the firm resolution to study psychological phenomena exclusively on empirical ground. Then, it appeared no longer necessary to struggle with

the centuries-old problems of monism, dualism, and hylemorphism; also, one could hope that the risk of further impingement of philosophical or theological postulates on experimental research had been definitively removed. Unfortunately, it took too long a time for the experimental psychologist to realize the exceedingly high price he had been paying to maintain this declaration of independence. He had thereby renounced willingly the unlimited possibilities for future investigations and had virtually exiled himself from his own inherited land. Ever since, he has been seen only too often wandering in border countries, trying to tackle marginal problems to gain acceptance among other scientists, and ultimately preparing the spoliation of the beautiful realm of psychology by foreigners who became increasingly interested in securing a part of it for themselves. The first invasion was marked by the appearance of psychophysics, psychophysiology, psychogenetics, psychopedagogy, psychotherapy, psychosomatic medicine, and even psychosurgery. Whatever had been left is now being rapidly appropriated by sociology and anthropology. After having been vassals to everyone we are now confronted with the alternative of either reconquering our lost unity, or withdrawing from the various fields where we have made significant contributions to the more peaceful academic world, in order to safeguard our dignity.

The immediate consequences of the fateful decision of building up "a psychology without a soul" did not reveal itself so fully at once. The psychologists of those days could not suddenly shake off the influence of their past training and, although they entered with enthusiasm on the new paths of empiricism, they did not feel compelled to relinquish the study of mental functions as they manifest themselves in Man. They were still interested in observing the functioning of intelligence, reason, will, memory, perception, imagination, and consciousness. However, they acquired rather soon the uneasy feeling of lingering over unsolid ground, where the construction of an elaborate synthesis could no more be attempted. It was then that the great *diaspora* occurred. The very few who, like Ribot and Janet for instance, still kept their deep interest in the study of human psychology, had to take refuge in the clinic where neuropathologists were happy to welcome them and requested their assistance for the establishing of a more discriminative symptomatology.

As to those who chose to remain in the experimental laboratory, they became literally obsessed by all that could reflect even the mere shadow of the *Homunculus* or of the *Ding an sich*. Overwhelmed by the shame of having been caught while indulging in "mythical" speculations, they hastily decided to do away with the mind without realizing that they were actually throwing out the baby with the bath water. Forgetting

that mental energy or psychic powers had traditionally been conceived as dynamic qualities of the human individual and not as substantial entities, they attempted rather awkwardly to solve the problem by substituting adjectives for substantives, by talking about intelligent or emotional behaviour instead of talking about intelligence or emotion. After innumerable futile disputes, though, a much more expedient solution was arrived at.

Until then, indeed, the dynamic conception of the mind had remained predominant and practically unchallenged. Opposing Plato's extremist idealism, Aristotle had proposed a moderate position which, although it was not always correctly interpreted, had become imbedded in our western tradition. After formulating the basic principle of realism: *Nihil est in intellectu nisi prius fuerit in sensu* (insisting, of course, that the intellect could perceive in sensorial representations some aspects of reality which remained beyond the reach of the senses), he did not hesitate to assume, as a logical consequence, that at birth the mind was like a smooth wax upon which nothing was engraved, a *tabula rasa*. However, he was keenly aware that, if this metaphor could be used conveniently to describe the passive receptiveness of the mind, such receptiveness had still to be conceived dynamically as a definite vital operation, namely, as the very operation of thinking. Therefore, he added this concise but meaningful comment, which is generally disregarded nowadays: *Istud pati, enim, est agere quoddam*. In modern time, although preserving this dynamic viewpoint, Leibnitz proved to be somewhat less careful in his reformulation of the Aristotelian principle when he submitted his own comment: *Excipe: nisi ipse intellectus*. Thus, in fact, he was paving the way for Kant's "categorical" interpretation. With all its vicissitudes, this great tradition had still enough vitality to permeate the increasingly less receptive field of experimental psychology under the aegis of Brentano's teaching and to give birth to the Gestalt School. However, in spite of its brilliant realizations, this group of enthusiastic workers could exercise only a transient influence, since its mode of thought was running counter to the whole trend of empirical simplification.

The Lockean tradition, which was slowly prevailing at least in Anglo-Saxon countries, ultimately provided the long awaited framework that seemed quite congenial to the straightforward experimentalist of this pioneering period. According to Locke, as Allport (1, p. 7) puts it clearly: "The intellect itself was a passive thing acquiring content and structure only through the impact of sensation and the crisscross of associations, much as a pan of sweet dough acquires tracings through the impress of a cookie cutter." There is no wonder, then, that the ex-

perimentalist could find little use for a mind which was a *tabula rasa* in the strict and integral sense of the term, and which was declared passive enough to be considered as a negligible variable in the study of human behaviour. On the other hand, Pavlov's reflexology had just begun to exert a powerful attraction and boldly offered unexpected possibilities for attempting a reinterpretation of such behaviour in terms of conditioning processes and of almost tangible concatenations of reflexes.

Such a unique opportunity for definitively discarding the inert, although still disturbing, mental variable seemed too nice for it not to be seized instantaneously. Hence, the human *person* vanished even from the psychological vocabulary and all approaches aiming at studying human behaviour as an integrated and global activity began to be labelled as "organismic." In standard textbooks, the totality of the human being was constantly designated as "the organism" rather than as "the person." Moreover, since the human organism appeared still to be endowed with some higher functions unsuitably complicating experimental devices, the subject-matter of psychology soon tended to be restricted to animal behaviour on the innocent basis of obvious practical reasons. The only thing which suggested that this sort of investigation had preserved a slight tint of humanity and was still preoccupied with human beings consisted in the fact that it was referred to as "comparative psychology." This designation, as it were, could hardly not be recognized as a misnomer by anyone perusing the literature even most casually. This indeed, contained scarcely anything else but reports on experimental studies of animal reactions to highly artificial laboratory situations, all of them presenting relative scientific interest but being merely pseudo-comparative.

As I have already hinted, during this period of harassing restlessness I have just tried to describe, the major contribution of the clinical psychologist consisted in keeping an alert inquisitiveness in regard to human personality. Considered as a practitioner rather than as a respectable scientist, because he could not find a suitable place in the laboratory team and had to associate with the clinic, the school, or the welfare agency, he had to work in complete isolation from his professional colleagues. He did not need to hear their suspecting or disparaging comments to fully recognize the inadequacy of his techniques for tackling the especially complicated problems he had in mind. However, he resisted the temptation of sinking into discouragement, and the very fascination exerted upon him by these problems became a strong incentive to undertake a patient and conscientious attempt at developing research tools increasingly corresponding to his needs. He preferred to be temporarily content with tentative results rather than to

let his whole approach be rendered inoperative through some crippling limitations and find himself compelled to renounce his long contemplated project. As his methods gained precision and more definite significance, he became bold enough to present his results and to propose his newly formulated hypotheses.

Among others, Mowrer (6), Rogers (8), and Wolff and Precker (12) have given us a rather impressive view of the tremendous progress recently made in this direction. This makes it unnecessary for me to enter into details which have already received careful consideration in their comprehensive studies. My only intention is to stress some few concepts of major importance, as they are concerned with what comes to be more and more generally recognized as the main cornerstones of a developing theory of personality.

Clinical psychology has emphasized strongly the existence of the *mental sphere*. Of course, its primary aim was not to establish or strengthen some spiritualistic metaphysical position through opposing the idea that "mind is brain." Its major and immediate preoccupation consisted in demonstrating that mental phenomena could also be studied experimentally, although not with the inadequate physical methods mentioned above; thereby winning a particularly important and vast territory for science. There is still a long way to go before the whole array of the adaptive and defensive functions of the psychic apparatus and their relations to the higher rational processes are thoroughly understood. However, a decisive step was made in this direction when the unexplored domain of the unconscious became accessible to scientific investigation. Now that we are clearly aware of the various dimensions of the mental sphere, its complexity no longer deters us from pursuing our observations, and a definite methodological orientation appears as a near possibility.

Another important contribution of clinical psychology is undoubtedly the rehabilitation of the concept of the *Self*, which Allport (1, p. 40) suggests calling the *Proprium* for the time being, apparently to avoid being accused of reintroducing the *Homunculus*. Reformulated on the basis of objectively collected and systematically analysed empirical material, this concept is slowly emerging from the halo of vagueness in which it used to be shrouded. There is little doubt that it will soon be accepted as the core of a badly needed structural interpretation of personality. And what appears to be especially comforting in this coming event is that such interpretation promises to be broad enough to account for normal and spontaneously arising patterns of behaviour as well as for deviational ones. Certainly, it will provide a more adequate frame of reference for a better understanding of the deep transformations

taking place in the course of the therapeutic process. But its main significance will likely derive from the new emphasis it is expected to lay on the more fundamental processes of becoming and of genuinely inventive adaptiveness. Seemingly, also, it is bound to modify our approach to the study of the individual personality, through drawing our attention to present life context and to anticipated future—immediate and remote—probably more than to past experiences and learned acquisitions. As one can easily suspect, this revision of the *Self* concept resting on empirical data quickly suggested the assertion of some other far-reaching positions.

The initial observation of the state of disorganization generated under the impact of internalized conflicts and of the reorganizing process occurring during the therapeutic experience evidently supplied the empirical starting-point of our recent structural approach. Unexpectedly, the thorough analysis of symptomatically determined patterns of regressive behaviour led to the strong assertion of the essentially *rational* character of normal mature behaviour. This, of course, involved the inescapable necessity of studying the complicated and obscure relationship existing between irrational impulses and rational demands, i.e. with the necessity of undertaking a fresh and courageous attempt at formulating a dynamic interpretation of human conduct. As substantial progress was being made along this line, the *Ego* became a major centre of attraction focusing the intense curiosity of numerous scientists. The former interest in the fundamental needs or drives or in the primary thought processes has been vanishing steadily in behalf of a revived interest in the so-called "higher," but for long totally neglected, functions of the human individual. Today, a fair number of investigations are centring around the problems of insight, adequate reality testing, logical and creative thinking, and rational attitudes towards goals and values. Thus, after almost interminable roundabout loitering, psychology seems to be at last reorienting itself in the direction of its core object of study, the behaviour of the normal personality.

In the meantime, other students were pursuing a parallel attack on the problem. Also, by a somewhat similarly circuitous way, they finally reached a no less encouraging and stimulating conclusion. After a thorough revision of traditional developmental and learning theories, they proposed rather timidly a first tentative structural interpretation. However, they soon discovered that personality could not be considered as a definitive and static product of a maturation process, but had to be conceived as something that is constantly and indefinitely in the making. Ultimately forced to tackle the most fascinating problem of its becoming, formulated in much broader terms than the ones we had until then

been familiar with, they arrived at a dynamic interpretation which, again, is unexpectedly based on the rediscovered *intentional* character of human behaviour. As it is easily assumed, such a viewpoint has tremendous implications. The human individual, instead of being merely reactive, is decidedly regarded as "self-propelling." Moreover, his activity is no longer encompassed in the closed and relatively tension-free system regulated by the principle of homeostasis. On the contrary, it may be equivalent to a persevering and extremely energetic pursuit of self-chosen values or freely determined aims, which keeps the individual under intense tension, possibly for his whole lifetime, without interfering in the least with his well integrated functioning.

It is necessary to finish this succinct historical review which has been condensed to the point of being reduced to skeleton proportion, but I wish to add one concluding remark. If one is looking at the Canadian scene to discover whether we are making a proper start in the most constructive direction, it is rather gratifying to observe that most of us have already seriously come to grips with the fundamental issues raised above and have unhesitatingly taken a firm stand. I would like to draw your attention to a few examples: (a) to Williams' (11) brilliant presidential address of last year and to Salman's (9) lucid paper, both aiming at situating psychology in regard to its multi-dimensional methodology; (b) to Hebb's (3) frontal attack on the classical "experimental" theory of motivation; (c) to Ferguson's (2) terse and highly suggestive revision of the standard theories of learning; (d) to Malmo and his associates' (10) long-term efforts at studying muscular tensions in connection with "higher" thought processes; (e) to Pinard and his associates' (7) outstanding discriminative study of intellectual functions; (f) to Mailloux's (5) attempt at clarifying the relationship existing between free will and psychic determinism.

This rapid and incomplete enumeration will suffice to indicate to a whole younger and encouragingly dynamic generation which direction we are taking. Personally, as a clinical psychologist, I will feel amply rewarded for my efforts if full success is finally attained in this unremitting attempt at helping psychology to become again a unified science and to ultimately recover its lost soul!

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THE MACLEOD REPORT ON CANADIAN PSYCHOLOGY¹

R. B. LIDDY
London, Ontario

IT MUST have required no small amount of courage for Professor R. B. MacLeod to undertake to survey the present status of psychology in Canadian Institutions of higher learning and to try to discover the aims of Canadian psychologists and the degree to which these aims are being achieved or frustrated. To do the job well needed more than courage. It needed wisdom and tact. That Professor MacLeod has brought to successful fruition the task he undertook will be immediately evident, I think, to everyone who reads his report. He had the necessary courage to begin the job and he was equipped with the necessary wisdom and tact to complete it and to complete it well.

In all his inquiries, so Professor MacLeod says, he kept certain questions in mind: (1) Is psychology being presented and accepted as a worthy contribution to a liberal education? (2) Has psychology "come of age" in the academic community? Is it now accepted as a legitimate science, biological or social, or are psychologists still having to fight for recognition? To what extent is psychology still identified with philosophy? (3) Are departments of psychology, in comparison with other departments, receiving budgetary support commensurate with their share in the educational work of the institution? (4) Is the content of psychology, as taught in courses and as represented by the interests of psychologists, unduly influenced by the success of psychologists in the applied fields (clinical, industrial, educational, government service, etc.)? (5) What is the influence on Canadian psychology of the proximity of the United States?

These are tricky questions and MacLeod makes no pretence of answering them with finality. He kept "probing for answers," as he puts it, and in his report, which he entitles *Psychology in Canadian Universities and Colleges*, he provides us with what he modestly calls his impressions. A satisfactory survey of the present status of Canadian psychology was not only a difficult task; it was, says MacLeod, for one man and in the time available, an impossible one. But there have been and still are a few choice souls who, as Berton Braley once wrote,

¹*Psychology in Canadian Universities and Colleges: a Report to the Canadian Social Science Research Council.* By Robert B. MacLeod. Ottawa: Canadian Social Science Research Council, 661 Island Park Drive. 50¢.

"specialize in the wholly impossible, doing things 'nobody ever could do.'" And Professor MacLeod seems to belong to this select group.

This short article is not an attempt to review the report. It is hoped that every member of the Canadian Psychological Association—certainly everyone engaged in the teaching of psychology in Canada—will read the report with care. This is merely an appreciative note written by one who, as he read, admired the clear style, the comprehensive sweep, the accurate summaries, and the stimulating suggestions of the author. Canadian psychologists will long remain indebted to Professor MacLeod, and to the Canadian Social Science Research Council which suggested and made possible the survey.

Part IV of the report consists of a number of conclusions submitted by the writer as "a crude balance-sheet of the impressions that stand out most clearly." He believes that psychology in Canada "is accepted almost universally as an indispensable component of a liberal arts curriculum" and that, "in the non-academic world psychology is no longer viewed with suspicion. Its usefulness has been demonstrated in the military services, in medicine, in education, in industry." These and other conclusions listed in the report on the positive or credit side may lead the Canadian psychologist to feel a degree of pride, indeed, of justifiable pride. But on the negative or debit side of the ledger there are conclusions far from pleasant, conclusions, however, with which few will find it possible to disagree; statements that will, or should, lead us to realize how very much remains to be done before psychology in Canada can really be regarded as having come of age.

Professor MacLeod believes that the increasing emphasis in recent years upon professional psychological training has "endangered" the contribution of psychology to liberal education. He fears that discovery of the principles by which people can be understood is being threatened by emphasis on hasty techniques for manipulating our fellow men. Over-enthusiasm for professionalism has as well this danger—it leads to inadequately trained students "with the result that most 'professional' psychologists in Canada are now rated and paid as technicians."

Professor MacLeod says that "Canadian departments of psychology are still sadly underequipped. Libraries and laboratories are woefully inadequate, and psychologists share the dishearteningly low economic status of their colleagues in other departments." This is, of course, only too true of practically all our departments of psychology. Whether the generalization applies without qualification to every one of our universities seems to me, however, a debatable question. The problem, of course, is to decide when a psychological library or laboratory is adequate.

Perhaps the most disturbing and challenging of the conclusions reached by MacLeod is that Canadian teachers of psychology, like those in other countries, are "confused as to the nature of their subject matter, as to the place of this subject in the curriculum, as to their proper role in the academic and non-academic community. . . . In Canadian educational institutions it is not clear whether psychologists are philosophers, natural scientists, social scientists, child specialists, mental testers, diagnosticians, therapists, or efficiency experts, or a strange combination of these" (p. 60). It seems to me to be unquestionably true that there is confusion in the public mind with regard to the subject-matter and the proper role of psychology. I am quite ready to agree that a certain amount of confusion with regard to these points exists in the minds of some of the teachers of psychology. But I should like to suggest, at the risk of leaving myself highly vulnerable to criticism, that most psychologists in Canada agree at least fairly well about the subject-matter of their discipline. And I am inclined to argue that those who differ from the majority, for example the neo-behaviourists, and the psychological physiologists (that phrase seems more apt than physiological psychologists) are not *confused* about psychology's subject-matter. They *differ*, of course, in their definition of psychology, or at least appear to—for the difference may after all be, as I hope it is, one of emphasis.

Our chief aim as psychologists is to discover the principles which will enable us to understand human nature. Most of us believe it necessary to investigate not merely the behavioural activities of living beings but conscious activities as well. And, unless one insists on reading mystical content into the concept, most of us believe that unconscious mental activities must not be ignored if we are to elucidate the principles basic to human understanding. Perhaps MacLeod would agree with this. He refers, however, not only to confusion about subject-matter but also to confusion about the proper role of the psychologist in the academic and non-academic community.

It may be that he intends to emphasize only that psychologists have a number of diverse interests and that accordingly their emphases vary. On page 38, after saying "one wonders whether any other discipline is as confused as to the nature of its own subject-matter [as psychology is]," he mentions different ways in which psychology is presented in Canada—as a professional field, as a social science, as a biological science and, especially in the smaller Roman Catholic institutions, as a branch of philosophy. Does this variety of interests and emphases mean that psychologists are confused as to the subject-matter of their discipline? Because some psychologists are child specialists, some mental

testers, some therapists, does it follow that there is confusion among the psychologists? Because psychologists assume different roles in the academic and non-academic community—a condition that seems to me both desirable and necessary—does it follow that psychologists are confused? We may classify our discipline sometimes as a natural science and sometimes as a social science, or think of it as partaking of the nature of each; we may even continue to speak of it as a branch of philosophy (as some in Canada and elsewhere do), without necessarily being confused about its subject-matter. But I am overemphasizing one point and perhaps distorting or misunderstanding MacLeod's meaning. We do need to recognize that there are differences amongst us, and if these differences suggest contradictions we need to do something about them. And that leads me to say a word about one of the excellent specific recommendations made in the last section of the report.

Professor MacLeod recommends that the Canadian Psychological Association seek funds to support a meeting of a small group of leaders in Canadian psychology. He undoubtedly knows from personal experience how valuable such a conference can be. The purpose of the Canadian meeting would be the discussion of the problems facing Canadian psychology. The topics suggested by MacLeod for the agenda of such a conference include the proper function and content of an honours curriculum in psychology, the standards for graduate degrees, and the relation between academic and professional psychology in Canada. The conference would examine also "the undergraduate curriculum in psychology as related to the ideal of liberal education," and "the relation between psychology and its neighbouring disciplines in the humanities, the social sciences, and the biological sciences." Perhaps I may comment briefly on these two points from MacLeod's suggestions.

Our undergraduate curricula in psychology differ considerably from university to university. This is inevitable and, I think, desirable. In some cases few subjects are offered even for honours students and still fewer for pass students. In other cases the honours course is heavily weighted with psychology and relatively few subjects from other departments are permitted. The honours course in psychology in our Canadian universities has, however, as a rule provided more opportunity for a reasonably wide and let us hope liberal education than some other honours courses. A liberal arts education, as contrasted with a technical or professional education, whether it be offered in an honours or in a general course, should provide the student with a fair sampling of subjects from the natural sciences, the social sciences, and the humanities. The conference MacLeod would like to have the C.P.A. establish would, of course, want to investigate as well the problem of how many under-

graduate pass courses should be made available, and what the content of these courses should be.

The other point has to do with the relation between psychology and its neighbouring disciplines. One sentence on page 38 led, on my part, to a reaction of mild astonishment and to the realization that, in spite of several decades of association with colleagues in other departments, I had probably not discovered what most of them thought about psychology. I realized, too, that I myself was probably much to blame for any existing misapprehension on their part.

The reading of the report has for me been a wholesome experience. I am glad that Bob MacLeod had the courage to accept the assignment. Those who asked him to do it knew in advance that he had the wisdom and tact to carry through a difficult survey and produce a report that would prove of great value to Canadian psychology.

INDIVIDUAL DIFFERENCES IN ORIENTATION IN PERCEPTUAL AND COGNITIVE TASKS¹

AUSMA RABE

University of Michigan, Ann Arbor

THE PURPOSE of this study is to investigate the relationships among individual reactions, in terms of *part-* and *whole-orientation*, in several perceptual and cognitive tasks.

Gestalt psychologists have demonstrated the importance of structural part-whole relationships in visual perception (1; 2, pp. 117-210; 9), and have applied the concept to other sense modalities and to cognitive behaviour (9, 10). However, the perception of part-whole relationships cannot be understood solely in terms of the stimulus situation. The perceiver's way of dealing with these relationships must also be considered. Individual differences in this function have been shown by Thurstone (7), Witkin (11), Witkin *et al.* (12), and recently by Newbigger (5).

The dependent variable of this study is the degree to which individuals differentiate parts and restructure them within the whole when dealing with a perceptual or cognitive task. "Whole-orientation" means strong adherence to the context and little differentiation of the parts. "Part-orientation" means relative independence of the context, and the tendency to differentiate and then reorganize the parts. The part-whole concept, as used here, is similar in terms of the main structural properties to Witkin's "part-of-a-field" tasks. Part-orientation appears similar to his "independence of the field," and whole-orientation to "dependence on the field" (12).

Subjects. The subjects, all volunteers, were 41 university undergraduates, enrolled in a psychology course. There were 29 women and 12 men, between the ages of 20 and 25.

TESTS, PROCEDURE, AND RESULTS

Test 1. Circle Size Illusion

Consider two circles, A and B, both of which have the same diameter. If the circle A is surrounded by a concentric ring C whose diameter is slightly larger than that of A, A is perceived as larger than circle B. Piaget, cited by Vernon, (8, p. 53) found this illusion to be stronger in children than in adults, the latter being more independent of the context (in this case the surrounding ring) in their size judgments. In terms of our concepts, overestimation of the size of the embedded circle would be a sign of whole-orientation, and veridical judgment a sign of part-orientation.

There were 3 standard and 11 variable stimuli, each drawn in the centre of a

¹Based on a thesis submitted in 1954 to the Faculty of Arts, Queen's University, in partial fulfilment of the requirements for the degree of Master of Arts. The assistance of Dr. A. H. Smith is gratefully acknowledged.

white card, 21 by 27 cm. The standards were black circles, each surrounded by a concentric ring .75 mm. thick. Ratios of the diameters of the circles to those of the surrounding rings were as follows: S_1 , 10/16 mm.; S_2 , 10/24 mm.; S_3 , 10/38 mm. The variable stimuli were black circles without a ring. Their diameters ranged from 8 to 13 mm., in steps of .5 mm.

Testing was carried out individually. The method of constant stimuli with two categories of judgment was used. The cards were presented manually, one at a time, against a yellow wall as a background. The exposure time was two seconds for each card, and the distance from the subject was one metre. The experimental room had ordinary daylight illumination. The subject had to make successive comparisons of each variable stimulus with the standard circles and report whether it was larger or smaller. The order of presentation of the standards and variables was randomized. Each subject made approximately 50 judgments with each standard. An error was the discrepancy between a subject's point of subjective equality and the diameter of the standard black circle.

Results. All errors were errors of overestimation, i.e., the point of subjective equality always exceeded 10 mm. Individual differences appeared in the degree of overestimation. The mean error and the range of errors were greatest with S_1 ; they decreased with S_2 and S_3 . Since the range of errors was greatest with S_1 , only these results were used in the correlational analysis presented later. Mean error, *SD*, and range of errors for S_1 were, respectively: .93 mm., .44 mm., and 0-2.00 mm.

Test 2. Gottschaldt Figures A and B

In this test a simple figure has to be located in a larger and more complex design. Individual differences in the ability to locate the embedded figures have been amply demonstrated (5, 7, 11, 12). In terms of our definition, persons with greater facility in extracting the embedded figures are more part-oriented than those who have difficulty in perceiving them.

The Gottschaldt Figures, their division, and the instructions were taken without alteration from Thurstone's factorial study (7, pp. 72-76). Subjects were tested as a group. The time limit for each part was as follows. Gottschaldt Figures A: Part I, 2 min.; Part II, 1 min.; Part III, 2 min.; Gottschaldt Figures B: Part IV, 3 min.; Part V, 3 min. The score in both parts was the number of correct tracings within the allotted time.

Results. Wide individual differences appeared in the facility with which the embedded figures were extracted from the larger configurations. For Series A of the Gottschaldt Figures, mean, *SD*, and range of correct tracings were, respectively: 20.9, 7.93, and 3-33. For Series B the corresponding values were: 13.75, 5.77, and 4-25.

Test 3. Minnesota Paper Form Board

This test (3) consists of two-dimensional diagrams of geometric forms. The correct form has to be identified from isolated, constituent parts. The correct response is an analytic one in the sense that the differentiated parts must be restructured to produce the whole. A large number of correct identifications should therefore indicate part-orientation, a small number whole-orientation.

Series AA of the Revised MPFB Test was given to the subjects as a group. Time allowed was 13 minutes instead of the usual 20 minutes. Otherwise the standard procedure and scoring were followed.

Results. There were wide individual differences in the number of correctly identified forms. Mean, *SD*, and range of scores were, respectively: 37.8, 8.98, and 20-54.

Test 4. Mooney Closure Test

The test (4) consists of incomplete black-and-white pictures of common objects. The correct percept of the object may or may not emerge with prolonged observation. It is not known precisely what happens in the process of perceptual closure, but it seems plausible that there must be differentiation of parts from the background, harmonizing of conflicting part-perceptions, and ultimate restructuring of the differentiated parts before closure can occur. If this is the case, it would justify the assumption that a large number of correct closures indicates part-orientation. Whole-orientation, with its defective differentiation of parts, would be indicated by a small number of correct closures. The number of correct closures is referred to as Part A.

A more cognitive reaction to this test was sought by supplying verbal hints for some of the items and not for others. A hint may perhaps be regarded as the cognitive counterpart of the fragmentary visual cues in the pictures. If so, we would expect utilization of the hints to vary with part- and whole-orientation, individuals who profited by them being more part-orientated than those who did not. This modification of the test is referred to as Part B.

Forty pictures from the test were used: Nos. 1-4 for practice and Nos. 5-41, omitting No. 11, as test items. On the basis of Mooney's data, the 36 test pictures were divided into two groups of approximately equal difficulty. For one group of 18 items hints were given; for the other group there were no hints. The hint was the correct name of a visible part of the picture, clearly printed in pencil at one corner of the picture, the choice of corners being randomized. The pictures concerned and the items named were as follows:

6. Mast.	13. Handle.	27. Ear.
7. Wheels.	15. Legs.	28. Arm.
8. Toes.	17. Eyes.	29. Table.
9. Funnels.	20. Stem.	31. Heads.
10. Neck.	22. Nose.	32. Dress.
12. Bench.	26. Wing.	33. Couch.

The instructions were those suggested by Mooney in the test manual. The test was given individually. The 36 pictures were shown to the subjects manually for 20 seconds each, at a distance of one metre. No mention was made of the hints. After the picture was shown, the subject wrote down his response.

Results. Part A of the test showed considerable individual differences in the number of correct closures. Mean, *SD*, and range were, respectively: 18.4, 5.08, and 6-28. The results in Part B were expressed as percentages of correct closures with the hint items calculated from each individual's total of correct closures. Thus, if a subject's total of correct closures was 27, and there were 9 closures with the hint items and 18 without hints, the score was 33.3%; a score of 50% would indicate that there were as many correct closures with hints as without. The percentages of mean, *SD*, and range of the hint closures were, respectively: 49.9, 8.01, and 33.3-66.7. The mean indicates that, on the average, the hints were not utilized to effect closures. Since the "equal difficulty" of the two groups of pictures was based on Mooney's averages, it is possible that there might have been some real individual differences in the extent to which the hints were utilized.

Test 5. Addition of Number Series

Wertheimer (10) has discussed thinking and problem solving in terms of the perceptual concepts of part-whole. Analogously, if we apply the concept of part-

and whole-orientation to visual situations, it may also be applicable to cognitive problems. Thus, one way of adding a series of numbers of a certain structure is by dealing with it as a whole, without examining its structural aspects. In terms of our concepts this means whole-orientation. Another way of solving the addition problem is by analysing the number series and noting the relations of single items to one another and to the whole structure. By our definition this indicates part-orientation. The number series used were designed to permit either type of solution; they were:

- (I) 1, 2, 3, 4, 16, 17, 18, 19
- (II) 17, 18, 19, 20, 21, 22, 23
- (III) 59, 58, 57, 56, 4, 3, 2, 1
- (IV) 96, 97, 98, 99, 101, 102, 103, 104
- (V) 1, 5, 9, 13, 17, 21, 25, 29, 33
- (VI) 9, 11, 13, 15, 17, 19, 21, 23, 25, 27
- (VII) 5, 10, 15, 20, 25, 30, 35, 40, 45
- (VIII) 1, 2, 3, 4, 7, 8, 9
- (IX) 2, 4, 6, 8, 10, 12, 14, 16, 18
- (X) 95, 90, 85, 80, 70, 20, 15, 10, 5
- (XI) 25, 23, 21, 19, 17, 15, 5, 3, 1, -1, -3, -5
- (XII) 17, 18, 19, 20, 21, 22, 18, 19, 20, 21, 22, 23
- (XIII) -8, -7, -6, -5, -4, -3, -2, -1, 11, 12, 13, 14, 15, 16, 17, 18
- (XIV) 15, 25, 35, 45, -50, 45, 55, 65, 75
- (XV) 16, 20, 24, 28, 32, 36, 40, 44

Owing to the balanced structure of each series, the sums could be obtained by adding the first and last items and multiplying by half the number of items. For example, No. I: $(1 + 19) \times 4 = 80$. Adding by this method was considered a part-oriented solution.

The test was administered individually, without a time limit. The subjects were handed a sheet with all of the series and instructed to look carefully at each row of numbers before adding it. After all series were added, they were asked to report on their method.

Results. Twenty subjects had no part-oriented solutions, and only two gave such solutions with all 15 rows. Mean and *SD* of part-oriented solutions were 3.22 and 5.10, respectively.

CORRELATIONAL ANALYSIS AND DISCUSSION OF THE RESULTS

To investigate the consistency with which individual performances were part- or whole-orientated, the product-moment coefficients of correlation were calculated. It will be remembered that high scores reflect part-orientation and low scores whole-orientation, except in Test 1 in which the reverse is true. Hence positive coefficients were expected in all correlations except those involving Test 1, which should be negative. Table I shows the coefficients obtained.² Six of them are significant at the 1% level and five at the 5% level. These are all in the expected direction. There is thus a fairly consistent relationship among individuals' reactions as measured by the various tests.

²Eta coefficients were computed for all scores, and the χ^2 test of non-linearity applied. There were no significant non-linear relationships.

TABLE I
PEARSON PRODUCT-MOMENT COEFFICIENTS OF CORRELATION
(degrees of freedom = 39)

	1	2, A	2, B	3	4, A	4, B
Test 1. Circle Size (magnitude of error)						
Test 2. Gottschaldt A (number of tracings)	.140					
Test 2. Gottschaldt B (number of tracings)	.058	.615**				
Test 3. MPFB Test (right minus 1/5 wrong)	.159	.685**	.543**			
Test 4. Mooney Closure A (number of closures)	-.334*	.416**	.413**	.419**		
Test 4. Mooney Closure B (per cent of hint closures)	-.228	.216	.323*	.299	.330*	
Test 5. Addition of Numbers (number of part-oriented solutions)	.027	.317*	.190	.368*	.301	-.173

*Significant at the 5% level of confidence.

**Significant at the 1% level of confidence.

The Circle Illusion, the most purely perceptual of the tests, correlates only with Mooney Closure A. This suggests that the structure of the circle test was too dissimilar from that of the other tests for the postulated relationships to hold. The three more complex perceptual tests, Gottschaldt Figures, MPFB, and Mooney Closure A, have the most consistent and the highest intercorrelations (all significant at the 1% level) in the matrix. As the tests become more cognitive, i.e. Mooney Closure B and Addition of Numbers, the relationships are smaller and less consistent. Apparently the structural property of part-whole has a subjectively different meaning as we move from the perceptual to the more cognitive situations. There is no good explanation why the two cognitive tests correlate with some and not with others of the highly intercorrelated perceptual tests. In case of the Number Series some of the inconsistencies may be due to the very skewed distribution of scores. The fact that Mooney Closure B correlates significantly with some of the tests seems to indicate that the more part-orientated subjects tended to utilize the hints, although, on the average, the hints were ineffective.

On the basis of the results of other studies (5, 11, 12) we would expect men to be more part-orientated than women. Contrary to this expectation, the median test (6, pp. 558-560) revealed no significant differences between the sexes, except on the MPFB Test. On this test

women were more part-orientated, i.e. obtained a higher score ($\chi^2 = 3.84$, $df = 1$, $P = .95$).

It is of interest to compare our results with those of Thurstone's factorial study (7, pp. 92-95). In addition to the Gottschaldt Figures A and B, his Street Gestalt Completion Test, PMA Space Test, and PMA Reasoning Test are comparable to our Mooney Closure A, MPFB, and Addition of Numbers, respectively. The corresponding correlations are all in the same direction, but those of the present study are considerably higher in most cases. All of Thurstone's tests mentioned here, except PMA Reasoning, have high loadings on the "speed and strength of closure" factor; and, except the Street Gestalt Test, also on the "flexibility of closure" factor. It seems possible that what we have called "part-whole orientation" is related to Thurstone's two factors of closure.

SUMMARY

The consistency with which individual reactions were part- or whole-orientated was studied in 41 subjects by means of five perceptual and cognitive tests. Pearson product-moment coefficients of correlation of the scores indicated a considerable degree of consistency in the subjects' part- or whole-orientation in some of the perceptual tests. The performances were less consistent as the tasks became more cognitive.

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EXPERIMENTAL INVESTIGATION OF BEHAVIOUR IN SOCIAL SITUATIONS:

II. INDIVIDUAL BEHAVIOURAL EFFECTS OF CHANGE IN GROUP ATTITUDE FROM OPPOSITION TO COOPERATION¹

VLADIMIR CERVIN²

University of Toronto

THIS PAPER reports a study of the effect on individual behaviour of a change in group attitude. The change in this experiment was from opposition to a member (subject) to cooperation with him. Each subject had to make up a story in cooperation with the two other members of the group, both of whom, by previous arrangement, warmly approved all his suggestions. This was in sharp contrast to the preceding situation reported in the first paper of this series (2), when they had consistently disagreed with him.

The theoretical frame of reference was reported in the previous paper (2). Recapitulating briefly, individual performance of subjects in a group situation is regarded as a function of their personalities and of the behaviour of other group members. It is assumed that each social situation can be described sufficiently (for a particular purpose) by describing a few of its independent dimensions. In this experiment we varied one personality variable, emotional instability, and one social variable, solidarity. Emotional instability is regarded as a complex state in which anxiety and neuroticism play important roles. It is conceptualized as the (high) excitatory potential with which the (emotionally less stable) subjects enter the experiment, i.e. it is a function of drive and differential habit strength (2), and is measured by a score on the scale of emotional instability. Solidarity or dissolidarity is defined as group approval or disapproval of a subject's performance in relation to a common task. It is conceptualized as positive or negative reinforcement of a member's response tendencies. It was experimentally controlled by prearranging the behaviour of some group members. This prearranged behaviour serves as its operational definition.

The change in group attitude towards the subject from opposition (dissolidarity) to cooperation (solidarity) is conceptualized as discontinuance of non-reinforcement of his response tendencies, and the beginning of their reinforcement. Under these conditions, one would

¹Research done under National Health Grants, Project no. 605-5-124 (to Professor J. D. Ketchum).

²The writer is indebted to Professor Ketchum for his advice and participation in this research.

expect that the excitatory potential behind the subject's responses, as measured by his response latency and the degree of his participation in the discussion, should increase. Making certain assumptions concerning the composition of the experimental sample (2), we predicted for the solidarity situation in which no response competition was expected:

1. Equal mean response latency for experimental (emotionally less stable) and control (emotionally more stable) subjects.
2. Both means smaller than under dissolidarity.
3. Larger latency variance for experimental subjects than for controls.
4. Larger mean participation for both experimental and control subjects than under dissolidarity.
5. Larger participation variance for experimental subjects than for controls.

PROCEDURE

Fourteen³ subjects who had participated in the opposition situation were put through a cooperative situation: seven experimental subjects, with high emotional instability scores, matched on several background variables with seven controls with low emotional instability scores (2). Groups of three were formed as before, with each subject facing two role-playing assistants, both strangers to him, except that they had just previously opposed him vigorously in discussion (see 2).

A large group TAT picture of four men in a room was shown to each group for 15 seconds, and they were asked to make up and agree on a story behind the picture. Assistants were instructed and trained how to behave. Their confidential instructions were "to avoid . . . suggesting any . . . stories; these must come from the subject. When subject makes a suggestion concerning a story, then approve the suggestion" (definition of solidarity). "The atmosphere should be warm and friendly throughout."

Performance of subjects was observed in three ways: (a) response latency, i.e. time elapsed from the word "go" to the moment when subject began to speak; (b) participation quotient (PQ), obtained from our Interaction Chronograph records, which gave the length of every speech of every person during discussion ($PQ = \text{total time speaking} / \text{total time silent}$); (c) total time the group took to decide on a story.

Reliability of measurements on recheck from recordings was .94 or better. Influence of extraneous variables was controlled, as far as possible, by matching and other means. All sessions took place in a room separated from the observation room by a one-way vision screen; voices could be transmitted both ways between the two rooms. All discussions were recorded on tape.

RESULTS

Data were analysed by *t* tests: differences between means of the two experimental situations by formulae from Walker and Lev (4, pp. 156, 157) for equal and unequal variances respectively; differences between

³Only seven pairs of Ss could be processed in this situation, out of 32 pairs who participated in the preceding situation. The reason was shortage of laboratory time. Selection, I think, can be regarded as random.

variances by the *F* test; and differences between matched subjects within each situation by the corresponding *t* tests for correlated scores (Tables I, II). These results are based on data for 32 pairs of subjects in the dissolidarity situation,⁴ and seven in the solidarity situation. They were found to be the same as the results based on data for the seven pairs who participated in both situations. Analysis of variance was not applied because of differences in variances (in this design it would be wasteful to transform the data to equalize the variances).

1. Table I shows that the mean response latencies under solidarity, as expected, do not differ for experimental and control subjects.

TABLE I

BEHAVIOUR INDICES FOR TWO LEVELS OF EMOTIONAL INSTABILITY AND SOLIDARITY
WITH PROBABILITIES OF DIFFERENCES BEING NOT SIGNIFICANT

Behaviour index	Solidarity dimension		Probability
	Dissolidarity	Solidarity	
<i>Response latency (seconds)</i>			
Means for { exp. Ss (less stable)	31.7	7.0	<.01
control Ss (more stable)	25.19	9.8	<.01
Probability	>.05	>.05	
Variances for { exp. Ss	1035.0	40.0	<.01
control Ss	400.0	79.0	<.05
Probability	.01	>.05	
<i>PQ (length of speech/length of silence)</i>			
Means for { exp. Ss	.53	.85	>.05
control Ss	.62	.92	.03
Probability	>.05	>.05	
Variances for { exp. Ss	.0514	.246	<.05
control Ss	.0759	.239	>.05
Probability	>.05	>.05	

2. Both means, as expected, are smaller under solidarity than under dissolidarity (Table I).

3. Variances under solidarity are not significantly different between experimental and control subjects, which is contrary to expectation. They are however much smaller under solidarity than under dissolidarity.

4. Mean participation quotients are larger under solidarity than under dissolidarity, as expected.

5. Participation variances, contrary to expectation, are not significantly different between the experimental and control subjects.

⁴Latencies for the dissolidarity situation (Table I) are taken from the first paper in this series (2).

DISCUSSION

The differences found between the two situations representing the extremes of the solidarity dimension confirm the predictions. One should note, however, that these differences can be attributed to the variation of solidarity only if the assumption is made that the habit strengths behind subjects' opinions expressed in these situations were initially equal. It is impossible to guarantee that this is so, but the nature of topics selected for discussion indicates that the assumption is not unreasonable. However it should be tested experimentally.

Comparing the performance of the two categories of subjects within each situation, one notices an absence of the expected differences between experimental and control *subjects* in the solidarity situation (3 and 5 above), whereas under dissolidarity experimental subjects tended to be more variable than the controls. It is interesting to compare this result with *group* behaviour indices. Table II gives the group mean times per task under dissolidarity and solidarity. With respect to variances, the group results are the obverse of the individual results: groups containing experimental and control subjects differ under solidarity, but not under dissolidarity. Thus, in the solidarity situation, the differences expected between individuals appeared between groups, while in the dissolidarity situation the differences expected (and found) between individuals disappeared on the group level.

TABLE II
MEAN GROUP TIMES PER TASK IN SOLIDARITY AND DISSOLIDARITY SITUATIONS
WITH PROBABILITIES OF DIFFERENCES BEING NOT SIGNIFICANT
(units are periods of 5 seconds)

	Dissolidarity	Solidarity	Probability
Means for groups { with exp. Ss	63.0	36.8	> .05
{ with control Ss	58.5	33.4	< .01
Probability	> .05	> .5	
Variances for groups { with exp. Ss	1811.0	729.5	> .05
{ with control Ss	1825.0	159.0	< .01
Probability	> .05	< .05	

One should not read into these results more than they contain, but they suggest certain possible interpretations with implications for future research.

1. First, one may look at the data from the point of view of *social groups*, defined as multi-individual entities capable of collective action (1), though in a somewhat more restricted sense than individuals. Perhaps, predictions of behaviour should be made in terms of behavioural units, rather than individuals or groups involved in a situation, with the

expectation that under certain conditions (e.g. solidarity) such units will contain more than one individual, while under others (e.g. dissolidarity) they may contain only one.

2. One may also look at the data from the point of view of ego psychology and *ego boundaries*. Lewis (3) has shown in a series of experiments that ego boundaries can encompass not only "what relates to [(S)] himself" but also, in some situations, "the needs of other selves or 'egos.'" From this point of view our data might be interpreted as follows. Under conditions of dissolidarity, when the subject was "on his own," his ego might have included only his own personality; in this case differences appeared between individuals. Under solidarity, when he was a functional part of the group, his ego might have included also the other two members of the group, and in this case group differences replaced individual differences.

This observation concerning behaviour differences and ego boundaries is offered, not to suggest any mystical cause-effect relation between the egos of group members and group behaviour, but as a possible way of determining ego boundaries operationally. If both individuals and groups may act, under appropriate conditions, as behavioural units, then the size of the behavioural unit manifested (as in the present findings) may serve as an operational definition of the ego boundaries of the group members. Ego boundaries in this sense may be an important factor in determining the cohesion of groups, one of the central problems in group dynamics.

SUMMARY

This experiment was designed to test predictions concerning the behaviour of subjects in a solidarity situation (group approval of a member) as compared with a dissolidarity situation (group opposition to a member). It was expected that the excitatory potential, as measured by subjects' response latency and degree of participation, would increase in the solidarity situation for all subjects, and that the emotionally less stable subjects would show more variability than the emotionally more stable. Seven experimental subjects (emotionally less stable) and seven controls (emotionally more stable), who had just participated in a dissolidarity experiment, were individually put through a solidarity situation as members of groups of three, which comprised one subject and two role-playing assistants privately instructed to approve everything the subject said. The group task was to make up a story about a TAT picture.

Hypotheses about differences between the two situations were confirmed, but hypotheses concerning greater variability of experimental

subjects were not. On the individual level the solidarity situation had a levelling effect on all subjects, whereas under dissolidarity the variability of experimental subjects was larger than that of controls. On the group level the results were the obverse: in the solidarity situation groups containing experimental subjects differed from those with controls, whereas in the dissolidarity situation they did not. Some interpretations of these results are offered in terms of group theory and ego theory.

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ORGANIZATION IN EMOTIONAL AND MOTIVATED BEHAVIOUR¹

DALBIR BINDRA
McGill University

THE CONCEPT of "organization" is used both ambiguously and loosely in current psychology. Pending a precise and explicit statement of the various connotations of organization, let us avoid confusion by defining, even though roughly, the terms *organized* and *disorganized* for purposes of the present discussion. These terms are not used here to refer to the adaptiveness or survival value of behaviour. Rather, in the present analysis, they refer to the extent to which the organism's behaviour consists of either a stable sequence of responses or responses that lead to a stable outcome (goal), or both of these. Organization in behaviour is thus defined here in terms of either one or both of (a) the stability of the *pattern* of responses involved, and (b) *goal direction*. Behaviour is organized to its highest degree when it is stable (hence predictable) with respect to both its sequence of responses and its outcome or goal. Behaviour's lowest degree of organization exists when it shows neither a table sequence of responses nor a stable outcome; responses are, as it were, disconnected, even though each individual response may be well-practised and readily available. Between these two extremes of the dimension lie various degrees of organization. This approximate definition of organization is adequate to provide an orientation for the present discussion.

While the concept of organization is used extensively in psychological discussions of perception, learning, and personality, it is in the area of "emotion and motivation"² that the concept has assumed a pivotal role.

¹This study was supported in part by the National Research Council of Canada (Grant A.P. 12).

²The words "emotion" and "motivation" are often used ambiguously, referring sometimes to behaviour which is emotional and motivated and at other times to the hypothetical (frequently neurophysiological) processes underlying such behaviour. In this paper the adjectives *emotional* and *motivated* qualify categories of behaviour; *emotion* and *motivation* refer only to hypothetical underlying processes. Where necessary, I have reworded the discussions of other authors to conform to this usage of terms.

Following accepted convention, the term *emotional behaviour* is used here as a collective name for the behaviour of anger, anxiety, jealousy, joy, and the like, and *motivated behaviour* as a general label for phenomena such as hunger, sex, and drug addiction behaviour.

However, the use of the concept in the treatment of this topic has led to considerable confusion. Psychologists have become involved in controversies over attempts at defining emotion and motivation in terms of organization, and over questions such as "Is emotional behaviour always disorganized?" or "Is motivated behaviour always goal-directed?" Leeper (7), Hebb (6), and Brown and Farber (1) have pointed out some of the contradictions and confusion in the literature, but so far no suggested remedy has proved effective.

The thesis of this paper is that it is possible to circumvent these difficulties by viewing organization from a developmental point of view. I argue that the developmental approach not only clarifies some of the current confusion, but also points to certain much needed experimental controls in research, and makes possible a consistent, systematic approach to the study of emotional and motivated behaviour. The argument is based on a developmental analysis of two instances of behaviour.

DEVELOPMENT OF ORGANIZATION

My first example traces the development of an instance of "emotional" behaviour, and the second example one of "motivated" behaviour.

Emotional behaviour. Hebb reports the following observations made on chimpanzees:

The experimenter, disguised with a grotesque false face and a change of clothing, approached each animal's cage wearing heavy gloves and acted the part of a very aggressive individual. . . . The first response by a number of animals was a generalized excitement and marked autonomic activity. An animal might be "friendly" and viciously aggressive, almost in the same breath, or show erection of hair and scream and yet solicit petting. Attack, flight and the friendly pattern alternated unpredictably. As the stimulus was repeated over a 5-week period, the autonomic activity decreased and one or other of the various patterns dominated. Eventually each animal's behaviour became predictable, but it appeared often to be a matter of chance whether the original disturbance would develop into fear, aggression, or (less often) friendliness [(5, p. 271)].

Considered developmentally, this analysis indicates that on the first few trials the animals were excited and their reactions were haphazard, lacking direction; on later trials the responses showed a sustained and directed pattern, with little sign of aimless activity. Observations of this kind have been made before. For example, Sherman's work (10) on "emotional development" in the human infant led him to stress the emergence of specific response patterns from an undifferentiated "aimless activity of most of the musculature." The above investigation of Hebb is reported here in some detail to show that, under suitable conditions, a similar course of development can also be seen in the adult animal. Essentially the same sequence of development appears in the following description of "motivated" behaviour.

Motivated behaviour. A need for morphine was established by Spragg (11) in four chimpanzees by giving them injections of the drug. When morphine was withheld from these addicted animals for a day or so they showed, in addition to the physiological withdrawal reactions, rather undirected general activity: pacing about their cages, crying, screaming, having temper tantrums, and so on. Although withholding the drug evoked this disturbed behaviour within only a few weeks of the beginning of the experiment, the animals did not show any tendency to "look for" injections of morphine till later. It was several weeks before they exhibited evidence of desiring morphine, at which time they tried to pull the experimenter into the injection room, adopted the injection posture, handled the injection paraphernalia, and showed other signs of wanting an injection. Here, too, as in the "emotional" example, we find the gradual emergence of directed patterns from responses which initially were neither patterned nor directed. It has been shown (3) that hunger behaviour follows a similar course of development.

It is clear that this sequence of development, from aimless activity, or with fluctuation of aim, to well-directed patterns of response, is not peculiar to behaviour usually labelled emotional (anger, fear, etc.) but can be observed also in so-called motivated behaviour (hunger, thirst, etc.). It appears that in both cases we are dealing with a developmental continuum ranging from unorganized or aimless activity to organized or directed and patterned responses.

To sum up, our developmental analysis suggests two generalizations: (a) Contrary to what is usually implied, we cannot distinguish between emotional and motivated behaviour (as conventionally labelled) in terms of organization. That is to say, the degree-of-organization dimension cuts across these traditional categories. (b) Organized behaviour develops from unorganized behaviour. Other conditions remaining constant, and with repeated exposure to essentially the same stimulus situation, the behaviour of the organism becomes gradually more organized.

IMPLICATIONS OF THIS DEVELOPMENTAL ANALYSIS

If organization and disorganization are regarded as the two extremes of a developmental continuum, a number of persistent and hitherto confusing problems can be clarified.

1. *The Young-Leeper controversy.* In view of our formulation of the unorganized-organized developmental continuum, it is apparent why some psychologists, for example Young and Leeper, could get involved in a controversy over whether emotional behaviour is organized or disorganized. Young looks upon emotional behaviour as primarily disorganized, and defines emotion as "... acute disturbance or upset ..." (13, p. 51). Leeper, however, considers emotional behaviour as organ-

ized, stating that "emotional processes . . . arouse, sustain, and direct activity" (7, p. 17). One suspects that the two authors do not mean the same thing by emotional behaviour. It seems that Young's emotional behaviour refers to the early stage of development involving aimless activity, whereas Leeper is talking about a later stage in the development from unorganized to organized behaviour.

This interpretation of the Young-Leeper controversy can be supported by experimental findings. For example, in one of his studies, Hebb (4) observed Tom and Dick, two young male chimpanzees, who occupied a strategic position in the Yerkes Laboratory. They could see the experimenter approaching before the experimenter could observe them. On the first day of the experiment, an unfamiliar test-object evoked considerable undirected excitement in both chimpanzees. But on later trials with the same stimulus, on observing the approaching experimenter, Tom and Dick would walk to the rear of the cage and sit calmly near the back wall of the cage. In deciding whether the behaviour of Tom and Dick is organized or disorganized, the crucial factor is the stage or trial at which it is observed: on initial trials the behaviour is undoubtedly disorganized, but on later trials it is organized.

2. *Control of developmental variables.* Our developmental analysis points to certain necessary experimental controls for research on emotional and motivated behaviour. Consider an investigator interested in determining the acts or response patterns evoked by a particular stimulus situation. Since, as we have seen, the observed response patterns vary with the developmental stage, no single act or single sequence of acts can adequately describe the behaviour of subjects at different times of observation, even when the stimulus situation remains the same. Thus, in the study in which the experimenter wore a disguise, the behaviour of chimpanzees in the early trials may be described as "generalized excitement," but this term would hardly be suitable to describe the more organized patterns of behaviour (withdrawal, attack, and so on) which emerged in later trials. Although the objective stimulus, probably intended to be a "fear stimulus," was kept constant, all phases of the observed behaviour could not be described simply as "fear behaviour." (To state *ad hoc* that the various observed response patterns were nevertheless "motivated or caused by fear" is to leave the descriptive level of discourse and to argue in a circle.) It appears, then, that there does not exist initially a general one-to-one relation between a given behaviour pattern (such as withdrawal or attack) and any specific stimulus situation (pain stimulation, frustration, etc.). Therefore, it is probably futile to look for such general relations. Any propositions regarding the effects of a stimulus situation on behaviour can be mean-

ingful only if we specify the extent of the subjects' previous exposure to that particular situation.

Even when the extent of past experience with the particular stimulating conditions is the same for all subjects, the response patterns shown may vary greatly from individual to individual. In the situation described earlier, with repeated exposure to the same objective situation, one chimpanzee may consistently withdraw to the rear of the cage, another may consistently attempt to attack the experimenter, another may vocalize until the stimulus is out of sight, and still another may deal with the situation by closing his eyes. The stable pattern of response which finally emerges in any particular case is quite different, not only from the pattern first displayed by the animal in that situation, but also from individual to individual. It seems reasonable to propose that the "choice" of the final stable response pattern depends, among other things, on the relative efficacy of previously established "means-end-readinesses" (12) of the particular animal. An animal that has been punished or unrewarded whenever it made an aggressive attack is less likely to adopt attacking as a means of meeting any new situation than an animal which has succeeded with such a mode of response.³

These considerations have an important implication for research. Only when the subjects have been repeatedly exposed to the same situation, and have come to display organized response patterns, is it possible to study directly the effects of other independent variables (such as brain stimulation, stress, hormone imbalance) on those response patterns. If the subjects have not had sufficient experience with a situation, and thus have not yet developed stable response patterns, it would not be possible to separate the effects of the independent variable from the developmental effects of repeated exposure to the situation. It is likely that the failure to control the influence of developmental factors is mainly responsible for many contradictory results concerning the effects on behaviour of frustration, "fear stimulation," stress, and so on, to be found in psychology textbooks and journals.

3. *Organization as a general dimension of behaviour.* We have seen that neither emotional nor motivated behaviour can be identified with organized or disorganized behaviour. At this point two alternatives present themselves. The first is that we disregard the conventional meaning of emotional and motivated behaviour and try to redefine

³We do not know what specific factors are responsible for individual differences in the stable response pattern finally emerging in a given situation. A fruitful line of research would be the investigation of the innate and experimental factors that determine *which* or *what type* of stable response pattern will finally emerge or be prepotent.

these concepts in terms of organization. Young (13) and Leeper (7) are not the only psychologists who have adopted this alternative. Recently, McClelland *et al.* (8) have defined motivated behaviour in terms that would identify it with organized behaviour. But getting psychologists to accept new definitions of old terms, no matter how logically sound they may be, is a difficult task; it is also unnecessary. Besides, definitions like those of the above authors would actually undo the progress made in the thinking about emotional and motivated behaviour. The gain made in delineating the somewhat more refined concept of organization would be lost by identifying it with the relatively vague concepts of "emotion" and "motivation." Thus, the suggestions of Young, Leeper, and McClelland *et al.*, instead of leading to precise definitions of emotional and motivated behaviour, would make the concept of organization equally vague.

The second alternative seems more desirable. It requires that we do not relate the concept of organization specifically to emotional and motivated behaviour, but rather that we consider the disorganization-organization variable as an independent, general *dimension* of behaviour. Such a general behavioural dimension could be used in describing what traditionally goes under the name of emotional and motivated behaviour, as well as other instances of behaviour. Degree of organization thus becomes one of the dimensions along which all behaviour patterns can be described. Of course, a number of other dimensions are needed to describe other aspects of behaviour patterns. Degree of *arousal* or *energy mobilization* is one such dimension already proving useful in systematizing results of experimentation on emotional and motivated behaviour (2, 9). The organization dimension could be just as useful. The aim of such a dimensional approach is to replace the vague, common-sense categories of behaviour by description in terms of precisely defined and general dimensions or variables.

It should be stressed that, with respect to organization, the dimensional approach makes no distinction between emotional behaviour and motivated behaviour. The main problem in the research area of "emotion" and "motivation" becomes one of studying the conditions that affect organization of acts which are conventionally labelled as emotional or motivated. Whether the acts under investigation are those traditionally discussed as "motivated" (for example, sexual excitement or food-seeking) or as "emotional" (for example, withdrawal or aggressive attack) is of no consequence from this point of view. Similarly, whether the stimulating conditions used in a study are those conventionally labelled "emotional" (for example, frustration, exposure to an unfamiliar situation, stress) or "motivating" (for example, food-deprivation,

hormonal imbalance, self-esteem involvement) also becomes irrelevant if we consider organization as a basic dimension of behaviour. The organizational aspect of both emotional and motivated behaviour, and indeed of other behaviours, could thus be systematically studied in terms of a single common dimension. The immediate problem is to refine the concept "organization" beyond what has been done for the purposes of the present discussion.

SUMMARY

A developmental analysis of two instances of behaviour, one traditionally categorized as "emotional" and the other as "motivated," suggests that: (a) the organization-disorganization dimension cuts across these two categories of behaviour; and (b) both organized emotional behaviour and organized motivated behaviour develop from unorganized general excitement. This analysis clarifies the Young-Leeper controversy; points to the type of experimental controls necessary for deriving significant generalizations about emotional and motivated behaviour; and suggests that only by regarding *degree of organization* as a general dimension of behaviour is it possible to approach systematically the study of emotional and motivated behaviour.

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THE INFLUENCE OF INSTRUCTION ON PERFORMANCE OF A COMPLEX PERCEPTUAL MOTOR TASK¹

CAROLYN L. STRAHM

University of Toronto

LEARNING may be inferred from several different aspects of the same behaviour. Changes in the measure of one of these would be expected to be accompanied by corresponding changes in the measure of another. For example, greater amounts of learning, as evidenced by increases in the rate of performance on a problem during a given practice period would generally be expected to accompany decreases in the number of incorrect responses (errors). A "problem" includes the sensory aspects of both the instructions and the apparatus; changing either of these would change the problem. Instructions in a particular experiment typically determine the problem or "set" for a given piece of apparatus. Thus, a problem might involve a pursuit rotor, with instructions to stay on target for a maximum length of time, or a complex coordinator, with instructions to make as many matches as possible.

Since instructions, or memory of them, are a part of the total stimulus pattern impinging on the subject immediately prior to and during practice, any change in them might be expected to influence performance. Investigators in the field of learning have long been interested in the nature and the extent of instructions which produce most efficient learning, i.e. increase the probability of correct responses and decrease the probability of incorrect or inappropriate responses. Since instructions regarding the appropriate responses are designed to aid correct movements, instructions on inappropriate responses might be expected to hinder errors.

The primary purpose of this study was to investigate the effect on performance of informing the subject that errors made during practice were being recorded. The following changes in performance were hypothesized:

1. A decrease in the mean level or rate of executing the problem.
2. A decrease in the mean number of total errors.
3. An increase in the mean latency of initial response to a new stimulus.
4. An increase in the tendency for the initial response to a new stimulus to be incorrect.
5. An increase in the length of time that a given error persists.

¹This work was conducted at the University of Toronto under Research Grant No. 265 from the Defence Research Board of Canada to Professor A. H. Shephard.

METHOD

Subjects

Two groups of ten male students enrolled in undergraduate courses at the University of Toronto served as subjects. None had any previous experience on the task.

Apparatus

A detailed description of the apparatus used, the Toronto Complex Coordinator (T.C.C.), has been given elsewhere by Shephard (2). A vertical display panel of 81 double-light assemblies arranged in a square block of 9 rows and 9 columns faces the seated subject. Each light assembly consists of a central green disc and an outer red ring. Separate electrical circuits permit the green disc and the red ring to be illuminated independently. Only one red light and one green light can be lighted at the same time. Illumination position of the red ring is determined by a 50-position stepping switch. The position of the lighted green disc is determined by the position of the subject's control unit.

The control unit used in this study is a simulated airplane-type control-stick linked to the display by two independent selsyn systems, by which the subject may "move" the green disc horizontally, vertically, or diagonally. The subject is required to centre the green disc within the lighted red ring for a period of at least .30 seconds. A new red ring is immediately presented. This is referred to as a "match."

Although the measures were recorded in 20-second intervals, they were later combined to give 10 one-minute intervals, referred to as "trials." Five measures of performance were used in the present study: number of matches, number of total errors, latency-match ratio, error persistence-total error ratio, and initial movement error-match ratio.

A "total error" is recorded each time the subject moves the green disc away from the red light. Total errors are recorded for the horizontal and vertical plane separately. For this report, horizontal and vertical total errors were summated to give a total error score.

"Latency" is regarded as the time between the presentation of a new red ring and the subject's first movement of the controls in the horizontal and in the vertical plane. As with total errors, latency is measured independently for each control. The latency-match ratios are obtained by dividing the total latency for both controls by the number of matches for each subject for a one-minute practice period.

"Error persistence" is a measure of the duration of an error. Error persistence-total error ratios are obtained by dividing the total error persistence by the number of total errors for each subject for a practice period.

An "initial movement error" is recorded each time the subject's first movement of the green light is away from the newly presented red light. These are recorded separately for the horizontal and vertical controls. Since the number of initial movement errors possible in any one trial is limited by the number of matches made in that trial, the initial movement error-match ratio is computed. This is obtained by dividing the total number of initial movement errors by the total number of matches for each subject for a practice period.

Procedure

Both groups were given ten minutes of continuous practice on the standard task in which movement of the control to the right moved the green light to the right, and movement away from the subject moved the light up the panel. All subjects were shown how to operate the apparatus and were instructed to make as many

matches as possible in the time allotted. The subjects in the Control Group were not told that errors were being measured. For the Experimental Group, the following statement was added to the instructions: "You should also know that we are keeping a record of the number of errors you make. An error is made every time you move away from the red light . . . like this." (Here the experimenter demonstrated two errors.) "But although we are recording your errors, it is extremely important that you try to make as many matches as you can."

RESULTS

The performance of the groups in terms of the five measures is shown in Figure 1. A trend analysis for each measure was used to determine the significance of the differences between the pairs of curves (1).

In the lower left of the graph the mean number of matches for both groups is plotted against number of trials. The curve for the Experimental Group is consistently and significantly lower than that of the Control Group ($F = 55.6$, with 1 and 180 degrees of freedom, $P < .001$).

At right centre the mean latency-match ratio is plotted against trials for the two groups. The curve for the Experimental Group is consistently and significantly higher than that for the Control Group ($F = 12.46$, with 1 and 180 degrees of freedom, $P < .001$). At the top left the curve of the mean number of total errors for the Experimental Group is significantly lower than that for the Control Group ($F = 70.93$, with 1 and 180 degrees of freedom, $P < .001$). The curve at the upper right, of mean error persistence-total error ratios plotted against trials for the Experimental Group is consistently and significantly above the curve for the Control Group ($F = 27.0$, with 1 and 180 degrees of freedom, $P < .001$). At the lower right is presented the means of initial movement error-match ratios. The curve for the Experimental Group is significantly higher than the Control Group ($F = 29.0$, with 1 and 180 degrees of freedom, $P < .001$).

The results suggest that the knowledge that errors are being recorded as presented in this study will result in:

1. A decrease in the mean number of matches.
2. An increase in the mean latency of the response to a new red light.
3. A decrease in the mean number of total errors.
4. An increase in the mean time an error continues.
5. An increase in the mean tendency for the initial response to a new red light to be in the incorrect direction.

DISCUSSION

These results suggest that instructions which include some reference to error measurement have a markedly different effect on performance than the same instructions without the reference to errors. The increase in the number of initial movement errors per match and in the error

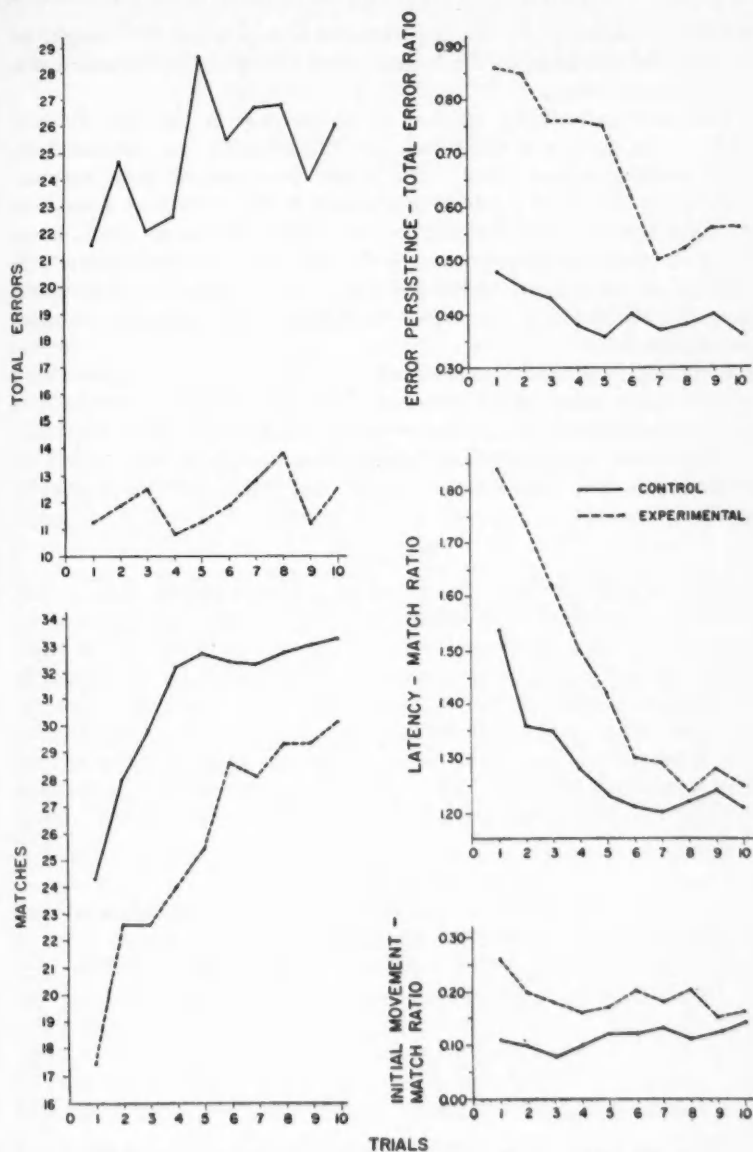


FIGURE 1. Five measures of the performances of the Control and Experimental Groups on the standard task.

persistence per error for the Experimental Group is not what might be expected on the basis of the hypothesized change in performance due to error instructions.

The error instructions resulted in an increase in the time interval between the making of the match and the subject's first movement towards making a new match. This longer time interval may have increased the likelihood of slight movements of the control as a result of muscular tension while deciding on the correct movement. Such movements would be as likely to occur in the right direction as in the wrong. This is to say that it may be that the greater the magnitude of the latency the more likelihood there is that the subject will make an incorrect initial movement.

The study does not suggest that all instructions regarding errors would have the same effect on performance. There are, obviously, many different emphases in instructions that may be placed on erroneous responses. Each different set of instructions may well have a different effect on performance. The instructions used in this study had the particular results indicated.

SUMMARY

This study investigated the effect on performance on the Toronto Complex Coordinator of instructing the subject that errors were being recorded. It was hypothesized that such knowledge would result in an overall change in performance such that there would be a decrease in the number of matches and in the number of total errors, and an increase in the latency-match, initial error-match, and error persistence-total error ratios. The hypotheses concerning the initial error-match and error persistence-total error ratios were based on the results of a previous investigation. Hypotheses with regard to all five measures were found tenable.

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RETROACTION EFFECTS IN THE EXPLORATORY ACTIVITY OF "BRIGHT" AND "DULL" RATS¹

WILLIAM R. THOMPSON

Queen's University

AND ASHER KAHN

McGill University

WHEN a rat is confronted with novel environmental stimulation, it exhibits exploratory activity which diminishes in amount as a function of time (3). The unfamiliar (provided it is not too unfamiliar) arouses curiosity which, in turn, elicits active investigation. This investigation itself reduces the newness of the stimulation, so that when the situation has become thoroughly familiar to the animal exploration ceases (1).

Now this decrement in exploration must mean that learning has taken place. It cannot be due simply to fatigue, since if the rat after exploring one situation for a certain time is then placed in another, it will again explore to an extent that depends on the similarity of the two situations to each other. If the second is very similar to the first, the animal is not faced with a completely new situation and will only explore a little; but if the two are completely different, it will explore the second as much as it did the first (4).

This being so, it seems likely that the laws of learning and memory must apply to exploratory behaviour. The major purpose of the present experiment is to show that one of these laws, retroactive inhibition (or facilitation), can be demonstrated for exploratory behaviour in the rat. In more operational terms, the problem may be stated as follows: if a rat is exposed to a situation (I) for a short period of time, then to another situation (II), then again to situation I, will the amount of exploratory activity elicited by the second presentation of situation I be affected by the interpolated situation II? If the amount of exploratory activity elicited in the second presentation of situation I is small, we may infer that the introduction of situation II has not interfered with recall of I; but if exploratory activity is high, then we infer that recall of I has been hindered by the interpolation of situation II.

The second purpose of the experiment is to discover if any differences either in amount or in orderliness of exploratory activity can be found between the McGill "maze-bright" and "maze-dull" rat strains (9). Although a previous experiment on the subject failed to yield any con-

¹This research was carried out at McGill Psychological Laboratory and supported by grants-in-aid from the National Research Council of Canada and the Rockefeller Foundation.

clusive differences (8), it was considered possible that they might appear in another kind of testing situation.

Finally, assuming that retroactive effects can be shown with respect to exploratory behaviour, a third question arises, namely, are these effects the same in bright and dull animals?

METHOD

Subjects. Thirty-six naive, hooded rats (approximately 90 days of age) of the McGill bright and dull strains (F6) were used. There were 18 bright (9 males, 9 females) and 18 dull (9 males, 9 females). These were divided into an experimental group containing 12 Ss, (3 bright males, 3 bright females, 3 dull males, 3 dull females), and a control group containing 24 Ss, (6 bright males, 6 bright females, 6 dull males, 6 dull females).

Apparatus. Two mazes were used for measuring exploratory activity, a flat black T-maze, and a light grey Y-maze. The arms of both mazes were 30 in. long by 3 in. wide, with walls 5½ in. high, and were covered by ¼-in. wire mesh. For purposes of measurement, each arm was divided into two 15-in. units. The number of such units traversed in a given time period was taken as indicating strength of exploratory activity. Animals were started at the base of the stems of the mazes. For "rest periods" between maze exposures, a flat black plywood resting cage, 10 × 8½ × 8½ in. was used. During the experiment, its wire mesh top was covered with black cloth to make it completely dark. The experimental room, 9 × 8 ft., was air conditioned and dimly illuminated by a 7½-watt bulb suspended 5½ ft. directly above the junction of the arms of each maze.

Procedure. The experiment was divided into three sessions. In Session I, all Ss explored the T-maze for 600 sec. In Session II, control Ss were placed in the resting cage for 1200 sec., whereas experimental Ss were placed in the resting cage for 300 sec., then in the Y-maze for 600 sec. exploration, then back in the resting cage for 300 sec. In Session III, all Ss again explored the T-maze for 600 sec. Thus the experience of the experimental and control rats was identical except for the interpolated activity in Session II. During each testing session, the path traversed by S was recorded exactly for each 100-sec. period separately. As mentioned above, scoring was in terms of the *number of 15-in. units* traversed. The reliability of this measure of exploratory activity was found by correlating the scores of the 24 control Ss in Sessions I and III on the T-maze. The Pearson product-moment coefficient was

TABLE I
MEAN NUMBER OF MAZE-UNIT TRAVERSALS MADE BY
BRIGHT AND DULL, MALE AND FEMALE, EXPERIMENTAL
AND CONTROL RATS IN SESSION I

	Experimental		Control	
	Male	Female	Male	Female
Bright	47.3	59.3	42.0	56.0
Dull	52.7	87.0	39.7	83.3

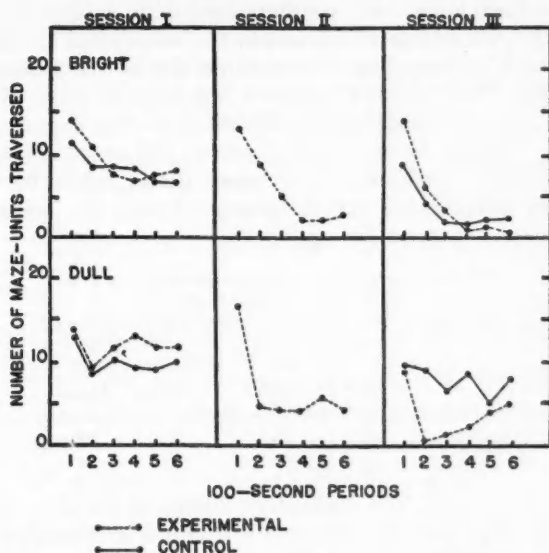


FIGURE 1. Number of maze-units traversed by bright and dull rats in the three experimental sessions. (T-maze in Sessions I and III, Y-maze in Session II.)

0.814, ($p < .001$). Food and water were available to all Ss at all times except during actual testing.

RESULTS

Curves of exploratory activity in each session for bright and dull, experimental and control rats are presented in Figure 1. Mean total scores for male and female, bright and dull, experimental and control Ss in Sessions I and III are shown in Tables I-VI. Detailed analysis of separate sessions follows.

(1) *Session I.* An analysis of variance was computed on total scores over the 600 sec. made by Ss in the different groups. There was no significant difference between experimental and control groups, but dull rats explored significantly more (i.e. greater number of unit traversals) than brights ($F = 5.91$, $p < .05$), and females significantly more than males ($F = 24.50$, $p < .001$). There was a significant interaction between brightness and sex ($F = 6.75$, $p < .05$), representing the fact that the bright-dull difference held only in the case of females. The means of bright males and dull males were not significantly different. However, inspection of Figure 1 indicates that the brights' exploratory curve de-

clines at a much faster rate than that of the dulls, so that the difference between the two groups is maximal in the second half of the session.² An analysis of variance done on scores over this half of Session I (Table II) confirms this. Females explored significantly more than males ($F = 5.42, p < .05$), and dulls significantly more than brights ($F = 7.10, p < .05$). Neither the experimental-control difference nor any of the interactions was significant. It is interesting that, while the curves of brights and dulls diverge with the passage of time, the curves of males

TABLE II
MEAN NUMBER OF MAZE-UNIT TRAVERSALS MADE
IN SECOND HALF OF SESSION I

	Experimental		Control	
	Male	Female	Male	Female
Bright	18.0	25.3	20.0	23.1
Dull	30.3	43.7	22.3	34.3

and females converge. The exploratory activity of females was initially much higher than that of males, and it declined at a much faster rate.

(2) *Session II.* Results similar to those reported above were obtained in Session II on the Y-maze. Again, Figure 1 indicates that experimental dull rats explored more than experimental brights. However, owing probably to the small numbers, an analysis of variance computed on these data yielded no significant differences.

It should be noted from Figure 1 that the recovery of bright rats in this session was relatively greater than that of dull animals. Brights explored 63 per cent as much as in Session I, dulls only 53 per cent as much. This fact presumably indicates that brights were able to discriminate between the two mazes more readily, hence the Y-maze was newer for them than it was for the dull animals, and consequently they explored more.

(3) *Session III.* Two types of analysis were made of results in this session, one using raw scores, the other using ratios between scores on the T-maze in Session I and in Session III. The purpose of the second type of analysis was to allow for any initial differences between Ss in Session I.

First, with raw scores (Tables III, IV), an analysis of variance yielded significant differences between males and females, and between brights

²In a previous experiment (8) a trend towards these results was found. Bright rats explored more in the first half but less in the second half of a testing session, than dulls. Although these differences were not significant, they lend support to the present data.

TABLE III
MEAN NUMBER OF MAZE-UNIT TRAVERSALS
(RAW SCORES) MADE IN SESSION III

	Experimental		Control	
	Male	Female	Male	Female
Bright	26.6	26.6	9.1	22.7
Dull	13.6	28.6	12.0	54.7

TABLE IV
ANALYSIS OF VARIANCE OF MEAN NUMBER OF MAZE-UNIT TRAVERSALS
MADE IN SESSION III

Source	Variance	F	p
Experimental-control	4.5	0.02	—
Bright-dull	861.0	4.39	<.05
Male-female	4053.8	20.75	<.001
Experimental variable \times brightness	1054.6	5.40	<.05
Experimental variable \times sex	846.2	4.33	<.05
Sex \times brightness	1347.8	6.89	<.05
Triple interaction	98.0	0.50	—
Within	195.5		

and dulls, but none between experimental and control Ss. Several interactions were significant: bright-dull \times experimental-control, male-female \times experimental-control, bright-dull \times male-female. The triple interaction was not significant. As indicated by the mean scores shown in Table III, the first two of these interactions represent the fact that bright rats and male rats of the experimental group explored *more* in Session III than bright controls and male controls, while dull rats and female rats of the experimental group explored *less* than dull controls and female controls. In other words, interpolated activity had an inhibitory effect on brights and males, and a facilitative effect on dulls and females. The third interaction means that the bright-dull difference held, as in Session I, only in the case of females.

Scores in each half of the session (Table V) were also analysed separately. For the first half, results were as follows: females explored significantly more than males ($F = 21.04$, $p < .001$), dull females significantly more than bright females ($F = 4.50$, $p < .05$), experimental brights more than control brights, but experimental dulls less than control dulls ($F = 13.30$, $p < .01$). No other differences were significant. Similarly, with the second half of Session III, females explored

TABLE V
MEAN NUMBER OF MAZE-UNIT TRAVERSALS MADE IN
FIRST AND SECOND HALVES OF SESSION III

	Experimental		Control	
	1st half	2nd half	1st half	2nd half
Bright				
Male	21.6	5.0	7.1	2.0
Female	24.6	2.0	15.5	7.2
Dull				
Male	6.6	7.0	7.3	4.7
Female	14.0	14.6	30.5	24.2

significantly more than males ($F = 11.23$, $p < .01$), dull females significantly more than bright females ($F = 5.95$, $p < .05$).

Thus these analyses indicate that the effects of interpolated activity on exploration in Session III appear mainly in the first half of the session, the bright-dull difference mainly in the second half, though even here it is largely confined to the female group.

The second type of analysis using ratio scores (Table VI) confirms the above results. With these scores the differential effect of interpolated activity on brights and dulls was even more marked. Females explored significantly more than males ($F = 6.92$, $p < .05$), bright experimentals more than bright controls, and dull experimentals less than dull controls ($F = 6.71$, $p < .05$). No other significant differences were found.

Finally, bright control Ss showed less recovery (39 per cent) from Session I to Session III than dull controls (54 per cent). In other words, although bright rats explored the T-maze less in the first session, they recalled it more readily after a 20 minute interval than the dull animals.

(4) *Orderliness of exploratory activity.* Following Montgomery (3), the sequence of maze-arm traversals made by each S was noted, and the

TABLE VI
PER CENT RECOVERY OF EXPLORATORY ACTIVITY IN
SESSION III

$$\left(\frac{\text{Session III}}{\text{Session I}} \times 100 \right)$$

	Experimental		Control	
	Male	Female	Male	Female
Bright	55.0	50.7	20.3	42.8
Dull	27.8	34.7	32.5	62.8

number of times found that S entered three different arms in succession ("unlike triads," e.g. ABC). These figures were then compared with the number of triads involving repetition of an arm traversal (e.g. ABA). The mean percentages of unlike triads traversed by bright and dull rats in each session are presented in Table VII. Since there were no differences between males and females or between experimentals and controls, these breakdowns are omitted. It was found that bright rats explored

TABLE VII
PERCENTAGE OF UNLIKE TRIADS EXPLORED BY BRIGHT
AND DULL RATS IN THREE EXPERIMENTAL SESSIONS

	Session I (T-maze)	Session II (Y-maze)	Session III (T-maze)
Bright	67	74	55
Dull	59	52	58
χ^2	6.20	3.90	0.57
p	<.02	<.05	—

in a more orderly way (had a higher percentage of unlike triads) than dulls in Session I and in Session II, though not in Session III.

DISCUSSION

In general, the above results indicate that exploratory behaviour, like conventional learning, is subject to retroactive inhibition and facilitation. It was found that the extent to which rats will re-explore a T-maze presented a second time after a 20-minute interval was affected by exploratory activity in a Y-maze interpolated between the two T-maze presentations. The effect was found to be inhibitory for dull rats, facilitative for bright animals. In other words, in Session III (first half), brights showed *less* recall (more exploration than controls), dulls *more* recall (less exploration than controls) of the T-maze. With the male and female groups, the situation was somewhat similar. Females, which explored more than males and at a faster rate, tended to show retroactive inhibition, males tended to show retroactive facilitation. However, since this sex-difference held only with raw scores in Session III, and not with ratio scores, we will confine our discussion to the retroaction effects shown in the bright and dull groups.

It is well established that retroactive interference is related to both stimulus and response similarity between the original and interpolated learning (2, 194-201). Where there is complete identity, only facilitation can occur; where there is great dissimilarity, neither facilitation nor inhibition will occur. Inhibition is maximal only with moderate similarity

between the two tasks. Now similarity obviously depends to a great extent on the learner. We might expect that under certain circumstances subjects able to discriminate readily between two situations (i.e. perceive them as dissimilar) will show different effects from subjects who cannot do so. It would seem that this was the case in the present experiment. The results thus hinge on the nature of the difference between the bright and dull rats.

These two strains have been bred selectively on the basis of performance on the Hebb-Williams maze (5), a measure designed to test short-term memory and adaptability in changing behaviour with a change in problem. Now, in a simpler way, the tests of exploratory behaviour used in the present experiment dealt with the same characteristics. The orderliness of exploration, its rate of decline during a session, and its recovery in a retest after an interval all indicate the speed with which an animal can familiarize itself with a situation. This can only depend on recall ability, other things being equal, and the greater orderliness in the exploratory behaviour of the bright rats, its rapid rate of decline, and its lower recovery rate in Session III (controls) in comparison with dull animals, all indicate the superiority of the brights in this respect. In addition, the higher recovery scores of bright rats (experimentals) from Session I to Session II indicate that they discriminated more readily between the T-maze and the Y-maze than the dulls.

Accordingly, the results both on the Hebb-Williams maze and in the present experiment supply grounds for supposing that bright animals are superior to dull rats in recall and discrimination ability. Consequently, it is not surprising that retroaction effects should be different for the two groups. Because the dulls were poorer in recall, the recovery scores of dull controls in Session III were higher. On the other hand, dull experimentals, because they did not readily discriminate between the T-maze and the Y-maze, generalized between them and hence showed less exploratory activity in Session III. For bright rats it was just the opposite. Bright controls, being superior in recall ability, showed less exploratory activity on the T-maze in Session III. Bright experimentals, however, unlike dull experimentals, did distinguish sufficiently between the two mazes, with the result that exploration of the second interfered with recall of the first. Consequently, their scores in Session III were higher.

Thus the results of the experiment are clearly related to the psychological characteristics of the subjects used. These, in turn, are dependent upon the genetic make-up of the animals. It is true that the brights and dulls differed considerably in this respect, being the product of six generations of selective breeding; they were, in fact, very different in

maze ability. That they differed *qualitatively* in the retroaction effects studied here must be due to what Scott (7) has called "magnification of a difference by a threshold." Scott has shown that, under weak illumination, *Drosophila* will crawl to or away from a light, depending on the possession of white or red eyes. This difference in eye-colour is conditioned by a single gene. But its phenotypic effect (positive or negative phototropism) is magnified by the threshold condition imposed by the experimenter, namely, luminosity. Likewise, the selected genotypic difference between the brights and dulls, while it produces a quantitative difference in certain situations (e.g. in a maze), produced, under the conditions involved in the present experiment, a qualitative difference in behaviour. Brights showed retroactive facilitation, dulls showed retroactive inhibition. Presumably this difference was due to the degree of perceived similarity between the two situations. Up to a certain degree, interpolated activity had an inhibitory effect on recall. Beyond this point, its effects were facilitative.

SUMMARY

The purposes of this experiment were to discover: (1) if retroaction effects could be demonstrated for exploratory behaviour; (2) if any differences in amount or orderliness of exploratory behaviour could be shown between the McGill bright and dull rat strains; (3) if any differences in retroaction effects in exploratory behaviour could be shown between McGill bright and dull rat strains.

Control Ss explored a T-maze for 10 min., and were retested after a 20-min. rest period. Experimental Ss were also tested twice on the T-maze, but ran a Y-maze interpolated between the two T-maze testings.

The results of the experiment justified the following conclusions:

1. Retroactive inhibition and facilitation can occur in exploratory activity.
2. Bright rats tend to show retroactive inhibition, dull rats retroactive facilitation, under the conditions of the experiment.
3. Bright rats explore less than dulls, but in a more orderly manner. The exploratory activity of bright rats declines at a sharper rate over time.
4. Bright rats recall more readily than dulls a situation they have already explored, and discriminate better between two situations.
5. Female rats explore more than males, and their exploratory activity declines at a faster rate over time.

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IRRELEVANT RESPONSES DURING EXTINCTION

IRENE MACKINTOSH¹
Fort Hays Kansas State College

FEW EXPERIMENTS on instrumental responses are designed to permit the measurement of responses other than those which the organism has been trained to make. Investigation of behaviour other than that which is reinforced has been in general limited to situations in which the appropriate, correct, or right responses are being acquired, while the random behaviour which constitutes the irrelevant, inappropriate, or incorrect responses is being extinguished. In the course of a larger investigation (1), record was kept of certain of the irrelevant or inappropriate responses which were made during the extinction of instrumental responses. Because of their theoretical and methodological interest these irrelevant response data are presented in this paper.

The experiment was designed to test the relationship between irregularity of acquisition conditions and differential resistance to extinction of instrumental responses. For the purposes of this paper, the only source of irregularity which need be considered is variation in the number of manipulanda on which instrumental reward responses could be made. Three groups of 16 rats each were given acquisition training on one, two, and three manipulanda, respectively. The number of positive reinforcements and the hours of deprivation were constant for all groups. After their varied acquisition training all rats were treated alike during extinction.

PROCEDURE

Subjects and Apparatus

The Ss were 48 naive rats ranging in age from 120 to 180 days at the beginning of training. Eight male and eight female rats were assigned to each of three groups on the basis of age and litter.

The apparatus consisted of a box 48 × 16 inches, which was divided into three compartments, the starting box, the response box, and the goal box. The three compartments were separated by vertical sliding doors. The response box contained three manipulanda, a chain, a horizontal bar, and a vertical bar, each of which, when operated under the appropriate circumstances, caused the door to the goal box to open. When this door opened, the rat could pass along a short runway to enter the goal box, where one of food (wet mash) or water was placed. Three selector switches enabled the experimenter to determine which manipulandum, when operated, would open the door. A "bypass" switch allowed E to open the goal box door without any one of the manipulanda being operated, and another switch prevented the opening of the goal box door when the selected manipulanda were operated. E manually reset the door to its closed position.

¹Now at the University of Oklahoma, Norman, Oklahoma.

The electrical wiring of the apparatus permitted automatic recording of response characteristics. A direct record of all appropriate responses (that is, responses made on manipulanda whose selector switches were "on") appeared on an electric counter, even when the door to the goal box was prevented from opening. Furthermore, each response made on any manipulandum, no matter whether or not that manipulandum was "on," was recorded on polygraph tape. For both acquisition and extinction, the polygraph tape provided a permanent record of the total number of responses in a trial, the number of responses made on each manipulandum, the order of use of manipulanda, and the latency of all responses.

As well as providing convenient operating and recording features, the electrical equipment added certain auditory stimuli with fixed relationships to the responses made. Whenever any response was made on any manipulandum, an audible click was made by a relay. In addition, whenever a response was made on the manipulandum which was appropriate for that trial, there was another, qualitatively different, click made by a counter. During acquisition the counter click accompanied the opening of the goal box door. Although the goal box door never opened during extinction training, all responses made on manipulanda which had previously been instrumental in attaining reward were accompanied by the counter click, while the relay click accompanied responses on all manipulanda, whether or not those responses had been instrumental in opening the goal box door. The two clicks were simultaneous.

EXPERIMENTAL DESIGN

Groups of 4 rats were handled as a unit during the experimental session. Throughout the entire experiment the trials were spaced, i.e. rat 1 was given trial 1, rat 2 trial 1 . . . rat 4 trial 1, then rat 1 was given trial 2. After the first day of preliminary training the subjects were under approximately 21½ hours' food and water deprivation at the beginning of each experimental session. Preliminary training consisted of training the rats to run from the starting box to the goal box when the doors were opened by E. This training, identical for all groups, lasted for 4 days.

Acquisition Training

For all groups, acquisition training began the day after preliminary training ceased and lasted for 7 days. All manipulanda were uncovered for all trials, but only one manipulandum was "on"; consequently, a response had to be made on the manipulandum which was "on" in order to open the door to the goal box.

Sixteen rats (Group B1) were trained to pull a chain which opened the door to the goal box, in which an appropriate primary reward was placed. Sixteen rats (Group B2) were trained to pull a chain or press a horizontal bar to open the door. Sixteen rats (Group B3) were trained to pull a chain, press a horizontal bar, or push a vertical bar. For rats trained on more than one manipulandum, only one response was appropriate on any given trial. The manipulandum designated to operate on each trial was randomly predetermined. All groups were given 90 reinforced trials. Thus, for Group B1 chain pulling was reinforced 90 times; for Group B2 each of chain pulling and bar pressing was reinforced 45 times; for Group B3 each of chain pulling, bar pressing, and bar pushing was reinforced 30 times.

On each acquisition trial, the rat was placed in the starting box and the door from the starting box was opened. When the response appropriate for that trial was made the door to the goal box opened. The rat entered the goal box and remained in it for 20 seconds. After the completion of the required number of trials for the day, food and water were placed in the home cages for 10 minutes.

Extinction Training

Extinction training began the day after acquisition training was completed. The extinction procedure was identical for all rats in all groups. Each rat was given 10 two-minute spaced extinction trials per day. The interval between the trials for each rat was approximately 10 minutes. Two minutes after the starting box door opened the rat was removed to his home cage. After the completion of 10 trials, food and water were placed in the home cage for 25 minutes. Extinction training was continued in 10-trial daily sessions until the criterion of four consecutive two-minute trials with no responses on appropriate manipulanda was met on two successive days.

Since all manipulanda were uncovered during all acquisition and extinction trials, it was possible for the rat to make responses on a manipulandum other than the one designated to operate. A response made on a manipulandum which, during acquisition, was sometimes instrumental for that particular rat in opening the door to the goal box, is referred to as an "appropriate" response. Responses made on manipulanda which were never associated with primary reinforcement are called "irrelevant" responses. Since Group B3 received acquisition training on all three manipulanda, there were no irrelevant responses for this group. For Group B1 there were two manipulanda on which irrelevant responses could be made, while Group B2 could make irrelevant responses on only one manipulandum.

RESULTS

The analysis of the extinction data, when only appropriate responses were considered, indicated that the greater the number of manipulanda upon which instrumental training had been given, the greater the resistance to extinction. For both number of responses on appropriate manipulanda and trials during which responses were made on appropriate manipulanda, the differences are significant well beyond the 1% level. These data were analysed without reference to irrelevant responses, i.e. responses made on manipulanda which were never associated with primary reinforcement.

During acquisition, Groups B2 and B3 made many responses on manipulanda on which training had been given but which were inappropriate for the given trial. Since these responses were not associated with primary reinforcement on the trials for which they were not operative, only periodic reinforcement was received for these responses; thus it is possible that some extinction of the appropriate responses was taking place during acquisition. In spite of this possibility, Group B3, which made the largest number of non-reinforced responses on appropriate manipulanda during acquisition, showed the greatest resistance to extinction.

During acquisition, irrelevant responses, i.e. responses made on manipulanda never associated with primary reinforcement, were very rare and occurred early in acquisition trainings. It would seem reasonable to explain these few irrelevant responses in terms of the random activity of rats deprived of food and water for 21½ hours.

TABLE I
MEANS AND STANDARD DEVIATIONS FOR APPROPRIATE AND
IRRELEVANT RESPONSES DURING EXTINCTION TRIALS

		Group B1	Group B2	Group B3
Appropriate responses	Mean	257.31	631.13	1105.44
	S.D.	194.50	322.25	835.91
Irrelevant responses	Mean	74.25	15.60	—
	S.D.	57.65	19.74	—
Total responses	Mean	331.56	646.73	1105.44
	S.D.	224.81	336.59	835.91

During extinction, however, the number of irrelevant responses was too large to be accounted for in terms of random activity. Table I presents the means and standard deviations for both the appropriate and the irrelevant responses for each group. The fact that the mean number of irrelevant responses for Group B1 is as high as 74.25 would seem to have both theoretical and methodological significance. One question which merits consideration is whether the differences between groups are significant when all the manipulandum responses made during extinction are included in the comparison. Table I reveals that even when all responses, irrelevant and appropriate, are included, Group B3 (trained on three manipulanda) made more responses to the extinction criterion than did Group B2, (trained on two manipulanda), and that Group B2 made more responses than Group B1 (trained on one manipulandum). The differences between groups, however, are significant only at the 5% level, as compared with well below the 1% level for appropriate responses only.

The distribution of irrelevant responses during extinction is also of interest. Figure 1 is a Vincent curve showing the mean number of irrelevant responses made per tenth of extinction by the rats in Groups B1 and B2. The curve indicates that, in general, irrelevant responses first increased and then decreased as extinction training proceeded. The mean number of irrelevant responses per Vincent trial started at 3.36, increased to 6.47, and terminated at 2.09.

DISCUSSION

As Table I indicates, Group B1 made approximately five times as many irrelevant responses during extinction as Group B2. Group B1 had two

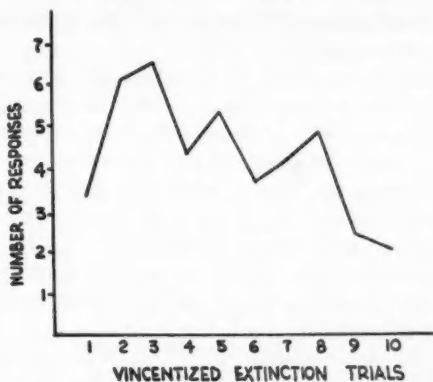


FIGURE 1. Mean number of irrelevant responses per Vincent trial made by 32 rats (Groups B1 and B2) during extinction. For half of the rats there were two available manipulanda upon which irrelevant responses could be made, for the other half only one manipulandum upon which irrelevant responses could be made.

available manipulanda upon which irrelevant responses could be made, while Group B2 could make irrelevant responses on only one manipulandum. The question arises whether the number of manipulanda available is the only factor determining the number of irrelevant responses. In other words, would some of the appropriate responses which were made by Group B3 have been made on inappropriate manipulanda had these been available? If so, then it would seem that many of the responses considered "appropriate" in this and other experiments, are in the same category as the responses here termed "irrelevant."

The large number of irrelevant responses must be considered in relation to one of the hypotheses which was being investigated. This hypothesis was that the greater the number of types of response which have been instrumental in obtaining primary reinforcement during acquisition, the greater will be the number of responses before the extinction criterion is reached. The prediction concerned only the number of appropriate responses to the extinction criterion. For Group B3, a response on any of the three manipulanda during extinction was taken as an indication that extinction had not yet occurred. For Group B1, however, operation of the two manipulanda upon which training had not been given was not interpreted as failure of extinction. The real issue is whether or not the irrelevant responses should be interpreted as indicating the existence of a response tendency which has generalized to manipulanda upon which no training was given. If so, extinction can-

not be considered complete until the generalized or irrelevant responses cease, as well as the appropriate ones.

With this possibility in mind, it is reasonable to suppose that the animals trained on fewer manipulanda would have continued to make responses on additional trials. Unfortunately, how many more trials was not determined, because the design of the experiment was such that the rat was dropped from the study when the set criterion, based on appropriate responses, was met. The data, as they were collected, do, in fact, show that more irrelevant responses were made when two inappropriate manipulanda were available than when only one inappropriate manipulandum was available. While the possibility must be admitted that the number of available alternative responses may be a factor in how irregularity of manipulandum conditions affects extinction, some of the observations in the present study suggest that this feature cannot account for the entire effect. Table I shows that the difference between Group B1 and Group B3 is extremely large, even when the irrelevant responses and appropriate responses made by Group B1 are combined. Hence the number of additional responses required by Group B1 would have to be correspondingly large, if the differences between the groups were to be eliminated. Figure 1 indicates that, in general, by the time the extinction criterion was reached, relatively few irrelevant responses were being made. That the differences between the groups would have been eliminated had extinction training been continued until no irrelevant responses were made is extremely unlikely.

The fact that a large number of irrelevant responses did occur is a phenomenon which in itself deserves study. This experiment differed from most investigations of extinction in that it permitted the measurement of irrelevant responses; the research, however, was not designed to investigate irrelevant responses *per se*, and permits only tentative conclusions concerning them. The initial rise in the number of irrelevant responses may be explained tentatively on the basis of secondary reinforcement generated by the clicks which accompanied the responses, while the subsequent fall in the number of irrelevant responses could be due to extinction. The irrelevant response phenomenon requires investigation as a function of similarity, since the role of generalization is probably important. One speculation about the phenomenon concerns its possible relation to frustration. Under frustration it has been observed that organisms frequently resort to regressive behaviour. The results of this investigation would seem to indicate that, when frustrated by the non-reinforcement of a learned response, organisms may make responses somewhat similar to the learned one, if the mechanisms for making these alternative responses are available. At any rate, it is apparent that

more empirical information is required. Moreover, in the absence of a full study of the phenomenon, further research should take into account the possibility of the occurrence of irrelevant responses with some functional meaning.

SUMMARY

Three groups of rats were given instrumental reward training on one, two, and three manipulanda, respectively. For groups trained on one or two manipulanda, additional manipulanda were available during both acquisition and extinction. During extinction many "irrelevant" responses were made on the manipulanda which had not been reinforced during acquisition. This phenomenon, which has not yet been specifically investigated, would seem to have theoretical and methodological significance.

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BOOK REVIEWS

Sigmund Freud: Life and Work. Volume One: The Young Freud 1856-1900. By ERNEST JONES. Toronto: Clarke, Irwin & Co. Ltd., 1953. Pp. xvi, 454. \$8.50.

The Origins of Psychoanalysis: Letters to Wilhelm Fliess, Drafts and Notes: 1887-1902. By SIGMUND FREUD (Eds. MARIE BONAPARTE, ANNA FREUD, ERNST KRIS, with Introduction and Notes by the latter. Authorized translation by ERIC MOSBACHER and JAMES STRACHEY). Toronto: Clarke, Irwin & Co., Ltd., 1954. Pp. xii, 486. \$8.50.

While as they lie in the grass of our neglect,
So many long-forgotten objects
Revealed by his undiscouraged shining
Are returned to us and made precious again

wrote W. H. Auden, not long after Freud's death in September 1939. Since then, in some real sense, much more than fifteen years' grass seems to have grown up, far more than a mere half-generation's hoard of beloved objects seems to have been forgotten. The mounting velocity of the historical process, nowhere so evident as in the humanistic sciences, progressively embeds the great lives and events of that time in the sort of crystalline permanence which one associates with times much longer past. Public attitudes to Freud and his work, moreover, passed during his lifetime, through cycles of enthusiasm and rejection of an amplitude usually encountered only posthumously. These features may in part account for Ernest Jones's unusual feat—the achievement of what seems likely to be a definitive biography—so soon after the death of its subject. A case might indeed be made for additional studies, either more narrowly intensive or more tangential; some have already appeared, though one feels that adequate evaluations in terms of general psychology and epistemology, to name but two fields, are still lacking; but surely none but a pupil and friend, himself an analyst of widely cultured penetration, could supply the fundamental, centrally focused survey of the man and his accomplishment. This Dr. Jones is giving us, and our gratitude should be considerable.

Meticulously, and properly, he suppresses his own personality and opinions; yet one of the most attractive features of the volume is the delicate yet highly distinct individuality which informs it. This is felt particularly in the treatment of such emotionally "charged" topics as Freud's courtship, his relationship with his in-laws, his difficulties with professional seniors, the painful ambivalence of his attitude to Fliess. It is an empathy far transcending mere observation, however detailed.

Details, indeed, seem to develop Gestaltwise from the global comprehension, and not the reverse. There is a common myth to the effect that successful psychoanalysis leaves a personality purged, indeed, of neurotic suffering, but at the same time flat, colourless, and uninteresting. This position becomes harder to defend after exposure to the combination of warmth and impartiality displayed in this work by Dr. Jones. As he says in his preface: "Immeasurably great as was my respect and admiration for both the personality and the achievements of Freud, my own hero-worshipping propensities had been worked through before I encountered him." The resulting attitude governs the work throughout: insight without fantasy, respect without servility, humour which irradiates but never cuts: in a word, wisdom.

These qualities are especially important in the gentle dissection of Freud's own psychopathology, which is inevitably a central motive in the whole study. Thus it seems equally inevitable that the dramatic climax of this first volume should be the account of the unique "self-analysis," beginning in 1897. It could be wished that Dr. Jones had devoted more than his brief chapter XIV to this "most heroic feat"; though, as he points out, promising further consideration in future chapters, it was a life-long process which by nature could never be completed. Here the volume of letters and notes so ably edited by Ernst Kris and his associates is of great value. Freud was a prolific and in a sense a compulsive correspondent. His own words, forged painfully in the heat of intellectual struggles over the critical years 1887 to 1902, vividly complement the more compressed and detached account given by Jones. Moreover Wilhelm Fliess, their recipient, played the necessary role of ambivalence, of simultaneous friend and enemy, more clearly than any other figure through Freud's life, and thus became the symbolic focus of much of the intellectual and personal conflict of those years. It was therefore no accident that the resolution through self-analysis of the conflicts of early childhood, particularly in Freud's relation with his father, coincided with the painful final break with Fliess. Kris's introduction is a valuable addition to Jones's account of this episode.

Taking the books together, then, we have the most revealing study yet available of the birth and infancy of psychoanalysis. Both works close at a natural epoch of transition: *The Interpretation of Dreams* is published, the analysis of Dora composed, the preliminary theory of psychic structure formulated. Henceforward, Freud will become more and more the master among pupils; the world must take account of him; the solitary phase of individual rumination is drawing to a close. The theme of unending rescrutiny and revision of past achievements, so characteristic of Freud's whole career and so closely related to the

patient regression of the analytic technique itself, has been firmly established. It is tempting to relate this process to the general cultural movements of the time. The world so vividly glimpsed through the medium of these two books is also the world of Proust, of the post-Wagnerian romantics, of the Impressionist painters, of great mathematicians—analysts all! Freud's early achievement stand out from these pages in the same context as the special theory of relativity, published in 1905. The process of transforming the matter of recollection into the energy of catharsis is in full harmony with other great discoveries of its time of origin.

PAUL CHRISTIE

University of Toronto

Social Psychology: an Interdisciplinary Approach. By HUBERT BONNER. Toronto: W. J. Gage & Co., Ltd., 1953. Pp. 439. \$4.25.

THIS textbook deserves serious consideration wherever social psychology comes early in the student's career. The author has succeeded admirably in weaving together materials from the three areas determining social behaviour: the biological heritage, the experience of social interaction, and the cultural context. The treatment is well organized and interesting, and—something too rare in textbooks—it is written in literate English.

Having said this, we must add that the integration of the fields is superficial, since the basic problems of social learning, social influence, and the acquisition of social motives get descriptive, not explanatory, treatment. In an author who acknowledges his debt to Gestalt psychology, it is strange to find learning described in Miller and Dollard's terms, perception allotted less than two pages, and "cognition," "field," "organization," and "Gestalt" missing from the index.

All told, the text gives an excellent picture of "what" happens in socialization, but is too defective on "how" it happens to appeal to advanced students. And whether even freshmen should read ten pages on the "neurotic . . . obsessive-compulsive . . . and paranoid" characteristics of the German personality may be seriously questioned.

J. D. KETCHUM

University of Toronto

Depression. Edited by PAUL H. HOCH and JOSEPH ZUBIN. New York: Grune & Stratton, 1954. Pp. x, 227. \$5.50.

THIS volume is the sixth in a series of volumes recording the proceedings of the American Psychopathological Association.

The presidential address by Zubin is in most respects the model of objective clarity which would be expected from this champion of methodology in the field of psychopathology. In attempting to make his brief survey of biometrics in psychopathology representative, Zubin considers the following five areas of investigation: (a) epidemiological studies, (b) genetic and environmental studies, (c) evaluation of the outcome of therapy, (d) experimental psychopathology, and (e) personality and body-type measurement in relation to psychopathology. Incidentally, although the central theme of the symposium is depression, Zubin's address—for the most part—is much more general in its orientation.

The Columbia-Greystone Project is cited as an example of how a therapeutic method may throw light on questions of theoretical value. On the basis of data from this lobotomy project, Zubin concluded: "Contrary to the traditional belief that the frontal lobes mediate association, learning, abstract behavior, etc., their surgical removal in part or in toto apparently has no effect on these functions."

While the negative findings in this project are striking, and such as to repudiate the more extreme claims of other authors, in the opinion of this reviewer, it is hazardous to conclude that complete removal of the frontal lobes has no effect on intellectual functions.

Near the end of the paper there are statements which seem inconsistent with the author's extreme caution in the matter of ascribing intellectual changes to operations on the brain. Zubin states: "Since perceptual deficiency is usually found in organic cases, perhaps those schizophrenics whose perceptual capacity is impaired give evidence, thereby, of an organic involvement which is generally not reversible, while those whose perceptual capacities are relatively unimpaired give no evidence of organic involvement and have a better prognosis." The concept of "organic involvement" appears too general to be useful. It seems important to further delineate the kind of "organic change" which could make the prognosis worse in schizophrenia. Furthermore, in consideration of lessons learned from the Columbia-Greystone Project, it seems important to specify the studies which have established that "perceptual deficiency is usually found in organic cases."

Other chapters of particular interest to psychologists are those by Spitz, Rado, Orbach and Sutherland, and discussions by Diethelm, Funkenstein, Goldfarb, and Hoch.

ROBERT B. MALMO

Allan Memorial Institute and McGill University

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- CANTOR, NATHANIEL. *The Teaching Learning Process*. Toronto: Ryerson, 1954. Pp. xvi, 350.
- COLLINS, V. M. E. *Approach to Psychology*. Toronto: British Book Service, 1953. Pp. viii, 130.
- EYSENCK, H. J. *The Scientific Study of Personality*. Toronto: British Book Service, 1952. Pp. xiii, 320.
- FRASER, D. C. *A Psychological Glossary*. Cambridge, England: Heffer, 1954. Pp. vi, 40.
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- MURPHY, GARDNER. *In the Minds of Men. The Study of Human Behaviour and Social Tensions in India*. Toronto: Clark, Irwin, 1953. Pp. xiv, 306.
- NUTTIN, JOSEPH. *Tâche, réussite et échec: Théorie de la conduite humaine*. Louvain, Belgium: Standard Boekhandel, 1953. Pp. ix, 530.
- PETRIE, ASENATH. *Personality and the Frontal Lobes*. Toronto: British Book Service, 1952. Pp. xii, 188.
- RALLI, E. P. (Ed.). *Adrenal Cortex*. New York: Josiah Macy, Jr. (Transactions of the Fourth Conference), 1953. Pp. 165.
- ROTHNEY, JOHN W. M. *The High School Student*. Toronto: Ryerson, 1953. Pp. xv, 271.
- SEIDMAN, JEROME N. (Ed.). *The Adolescent: A Book of Readings*. Toronto: Ryerson, 1953. Pp. xviii, 798.

*The inclusion of a book in this list does not necessarily preclude a review of it appearing in a later issue of the *Canadian Journal of Psychology*.

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